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THE ACCOUNTING REVIEW

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Tax-Motivated Expense Allocations by Nonprofit Organizations

Robert J. Yetman
The University of Iowa

ABSTRACT: Although nonprofit organizations are generally exempt from income taxation, they pay taxes on profits from activities unrelated to their primary exempt purpose. Congress intended this tax on unrelated business activities to prevent unfair competition with for-profit businesses and to raise revenue. In the aggregate, nonprofits report annual losses on their taxable activities of more than $1 billion on $4 billion of revenues. In contrast, they report aggregate profits of over $50 billion on their tax-exempt activities. Analysis of a database of confidential tax returns suggests that medical and educational nonprofits allocate expenses from their tax-exempt to their taxable activities to reduce their tax liabilities, although unfortunately it is not possible to determine the extent to which these allocations represent noncompliance with tax laws. In contrast, I find no evidence that charitable nonprofits engage in tax-motivated allocation behavior.

Keywords: *nonprofit organizations; unrelated business income tax; cost allocation.*

Data Availability: *Due to nondisclosure arrangements, data are not available from the author, but they may be obtained from sources identified in the text.*

I. INTRODUCTION

This paper investigates the relation between income taxes and expense allocations made by tax-exempt, nonprofit organizations. Although nonprofits are generally exempt from income taxes, they are subject to an unrelated business income tax on income from activities not related to their exempt purpose. For example, a university that

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This paper is part of my dissertation completed at The University of North Carolina at Chapel Hill. I thank my committee, Charlie Clotfelter, Julie Collins, Wayne Landsman, Mark Lang, and especially Doug Shackelford (chair) for their invaluable assistance and thoughtful mentoring. I would also like to thank workshop participants at the Universities of Georgia, Illinois, Iowa, and Oregon, and the Kennedy School of Government at Harvard University. The paper benefited from discussions with Ramji Balakrishnan, Jim Leisenring, Burt Weisbrod, and Michelle Yetman. The suggestions of Richard Sansing (associate editor), and two anonymous reviewers were particularly helpful. The financial support of the Arthur Andersen Foundation, the Center for the Study of Philanthropy and Voluntarism at Duke University, and the Cato Research Fund at The University of North Carolina is gratefully acknowledged.

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uses its basketball arena for private musical concerts must pay taxes on the net profits generated from the concerts. In computing its taxable income from a concert, the university may deduct reasonable expenses, which comprise expenses incurred only for the concert (e.g., parking attendants) and a portion of other expenses (e.g., electricity) that must be allocated between the taxable and tax-exempt uses of the arena. The differential taxation of related and unrelated activities provides nonprofits an incentive to shift expenses from tax-exempt-related activities to taxable-unrelated activities to reduce their tax liabilities.

Congress enacted the unrelated business income tax in 1950 in response to claims of unfair competition, such as those levied against the Mueller Macaroni Company that was tax-exempt because it was owned by New York University. Congressional records suggest two purposes for the tax: first, to prevent unfair competition with taxable businesses arising from nonprofits' tax-exempt status; and second, to raise revenue (U.S. House 1950).[1] In the aggregate, nonprofits report annual losses on their taxable activities of over $1 billion, a situation that has persisted for more than ten years and is unique among entities subject to income tax in the United States. In contrast, nonprofits report aggregate annual profits of more than $50 billion on their tax-exempt activities. Operating a truly unprofitable taxable activity would not only financially drain the nonprofit, but would also require exempt sources of income (such as donations) to subsidize the unprofitable taxable activities.

When expenses are not separable, such as joint expenses incurred for both tax-exempt and taxable activities, economic allocations across activities are not well defined (Demski 1994). One purpose of a cost accounting system is to allocate these nonseparable expenses. Prior analytical research suggests that managers will respond to tax incentives when selecting an expense allocation basis (Sansing 1998). The purpose of this paper is to provide empirical evidence on the extent that nonprofits respond to tax incentives by reallocating a portion of expenses from their tax-exempt activities to their taxable activities, and to measure the extent to which these tax-motivated expense allocations are responsible for the reported losses on nonprofit's taxable activities.

The analysis in this paper builds on prior research in nonprofit accounting, managerial accounting, and tax accounting to develop the expense allocation models. The study represents the first large-sample, firm-level empirical analysis of the unrelated business income tax, one of only four types of income taxes in the United States.[2] By using a firm-level dataset of confidential tax returns, the study overcomes some limitations of prior income-shifting studies that were generally unable to estimate firm-level allocations (e.g., Klassen et al. 1993; Collins and Shackelford 1995).

Using a database of confidential nonprofit tax returns, I construct a model that partitions total expenses between nonprofits' taxable and tax-exempt activities. The difference between my estimates of taxable expenses and those the nonprofits actually report on their tax returns is my estimate of the portion of expenses that nonprofits reallocate from tax-exempt to taxable activities. Hereafter, I refer to this difference as tax-motivated expense allocations. Ideally, my estimation procedure would partition the expenses between taxable and tax-exempt activities based on the resources each activity actually consumes. Lacking access to usage data, I partition expenses based on each activity's reported revenues. This procedure implicitly assumes that: (1) nonprofits do not shift revenues across tax-exempt and taxable activities, and (2) expenses are proportional to revenues.[3]

---

[1] Some observers suggest that unfair competition may not occur in perfect markets with perfect compliance. In this setting, the purpose of the unrelated business income tax shifts from preventing unfair competition to preventing inefficient production. See Hansmann (1989) and Sansing (1998) for a discussion of this issue.

[2] In addition to nonprofit organizations, income taxes are levied on individuals, corporations, and decedent's estates.

My analysis is based on a database that matches nonprofits' publicly available information forms (IRS form 990) with their confidential income tax returns (IRS form 990-T). In response to a written request, 703 nonprofits voluntarily supplied an average of 2.6 years of data each, creating a pooled cross-section of 1,824 matched sets of forms 990 and 990-T.

Results suggest that medical nonprofits in the sample responded to tax incentives by reallocating a mean (median) of $405 ($86) thousand in expenses from their tax-exempt activities to their taxable activities. Educational nonprofits reallocated a mean (median) of $101 ($45) thousand in expenses from their tax-exempt activities to their taxable activities. I find no evidence of tax-motivated expense allocations for charitable nonprofits. The majority of the tax-motivated expense allocations occur in categories that aggregate miscellaneous expenses, such as "other" expenses. I find no tax-motivated expense allocations in categories that are subject to third-party reporting to the IRS. The median estimated tax-motivated expense allocations are sufficient to produce the median reported losses on taxable activities for the sample of medical and educational nonprofits. To the extent that these results are representative of the population, they help explain the aggregate losses nonprofits report on their taxable activities.

Cordes and Weisbrod (1998) examined the relation between tax incentives and nonprofits' variable employee compensation expense allocations, and found that the portion of compensation expenses allocated to taxable activities exceed estimates of marginal expenses. However, their sample sizes are very small (i.e., 7 charitable, 12 educational, and 24 medical nonprofits), they ignore fixed expenses, and they examine only employee compensation expense. My study uses a significantly larger dataset (1,824 organization-year observations), considers both fixed and variable expenses, and examines all categories of expenses. In another study of expense-shifting in nonprofit organizations, Eldenburg and Soderstrom (1996) find that hospitals appear to shift contractual adjustments (the difference between a reimbursable amount and the costs actually incurred for a treatment) to activities that are more likely to be reimbursed from nonpatient sources. I adapt a portion of Eldenburg and Soderstrom's (1996) research design to construct the expense allocation models.

The next section discusses the institutional characteristics of nonprofits. Section III develops a model of tax-motivated expense allocations, Section IV discusses the data, Section V reports the results of the empirical analysis, and Section VI concludes.

II. INSTITUTIONAL CHARACTERISTICS OF NONPROFITS

Although the taxable portion of nonprofit revenues averages only 1 percent of their total revenues, taxable revenues are growing three times faster than total revenues. The number of nonprofits reporting taxable activities has increased from less than 5 percent in 1975 to more than 12 percent in 1995 (Meckstroth and Arnsberger 1998). In response to this growth, the IRS created a separate division for tax-exempt organizations and doubled the number of employees in the tax-exempt section.

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[3] In addition to tax-motivated expense shifting, nonprofits may also engage in revenue shifting by reporting a portion of their taxable revenues as nontaxable. The inability to construct reasonable expectations models of revenues prevents me from estimating tax-motivated revenue reallocations, omitting a potentially important source of tax-motivated activity from the study.

The individual states grant authority to operate on a tax-exempt basis. State approval of nonprofit status generally exempts an organization from state-level taxes, such as income, property, payroll, and sales taxes, but not from federal income taxes. A nonprofit may be exempt from state-level taxes but not from federal income tax.[4] In practice, most organizations that receive state-level tax exemption also receive approval at the federal level.

Not all income earned by nonprofits is tax exempt. Nonprofits must pay state and federal unrelated business income taxes on business activity not directly related to their tax-exempt purpose. Typical sources of unrelated business income include laundry facilities, catering services, clerical services, condominium rentals, daycare, farming, pharmaceutical sales, costume rentals, parking lot development, and advertising revenues. Nonprofits with taxable revenues must file two IRS forms, the 990-T and the 990. The form 990-T, which appears similar to an income statement, reports taxable revenues and expenses and is not subject to public disclosure. The form 990, a publicly available annual information return, reports the organization's total operations, aggregating both tax-exempt and taxable activities. One cannot determine the portion of revenues and expenses pertaining to taxable activities from the publicly available form 990 alone.

Nonprofits often use existing facilities and personnel in taxable activities; therefore the nonprofit must allocate dual-use expenses between the taxable and tax-exempt activities for tax-reporting purposes. The only guidance the IRS provides to nonprofits is that they must allocate dual-use expenses according to a "reasonable basis" (Treasury Reg. 1.512[a]-1[c]).[5] Unfortunately, the IRS and the appellate courts disagree as to what constitutes a reasonable basis. An appellate court decision permitted a university to allocate joint expenses based on actual use rather than capacity available for use (*Rensselaer Polytechnic Institute v. Commissioner* 1984). For example, if a facility was used 100 days for tax-exempt purposes and 20 days for taxable purposes, the allocation percentage under the court decision is 20 /(100+20) or 17 percent, while the allocation under IRS rules is only 20/365, or 5 percent. The IRS has not acquiesced to the court decision.[6] This disagreement, along with limitations inherent in any model estimating the expenses of (or resources consumed by) a nonprofit's activities, makes it difficult to interpret the results of this study as evidence of noncompliance.

This study examines only nonprofit organizations exempt under Internal Revenue Code section 501(c)(3), which are those that can receive tax-deductible contributions. Nonprofits fall into one of three general categories: hospitals, schools, and other charitable organizations. These categories do not include pensions, social clubs, or local governments. Although religious organizations fall under the purview of 501(c)(3), they are not required to furnish the IRS with information reports and are excluded from the study.

III. A MODEL OF ORGANIZATION-LEVEL EXPENSE ALLOCATIONS

Model Development

Ideally, I would estimate expense allocations under both the IRS's and the Court's methods using detailed expense and facility use data, and then compare these estimates to the actual expense allocations reported on the nonprofit's 990-T. Without access to confidential expense data I must develop a model to estimate the expense allocations. I partition taxable and tax-exempt expenses using a two-step procedure. First, I use regression analysis to partition the variable expenses. Second, I partition any expenses that remain unallocated after the first step (ostensibly fixed expenses), based on average revenues.

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[4] For example, the American Automobile Association (AAA) is a nonprofit membership organization for state purposes, but does not meet IRS criteria for federal income tax exemption.

[5] Expenses that are directly attributable to an activity are not subject to allocation.

[6] The IRS has formally issued a non-acquiescence, a Technical Advice Memorandum, and a Private Letter Ruling on this point. See Private Letter Ruling 9149006.

To construct the model I make several assumptions. First, I assume that the relationship between revenues and expenses is constant within nonprofit types (i.e., medical, educational, and charitable). Second, I assume that taxable activity resource inputs and related sales revenues are proportional. Third, I assume that variable expenses are incurred on a marginal basis and that fixed expenses are incurred on an average revenue basis. Finally, I use a model to allocate expenses because I cannot directly associate expenses with any particular revenue source (except for fund-raising expenses, which are directly associated with donations revenue).

Estimated Variable Expense Allocations

To partition variable expenses between taxable and tax-exempt activities, I begin by estimating the following model in asset-scaled first differences:

$$\begin{aligned}\Delta\text{TOTAL\_EXP}_i = \alpha + \beta_1\Delta\text{TAX\_REV}_i + \beta_2\Delta\text{EXEMPT\_REV}_i \\ + \beta_3\Delta\text{INVEST\_REV}_i + \varepsilon_i,^7\end{aligned} \tag{1}$$

where ΔTOTAL_EXP is the change in total expenses from the form 990, less fund-raising expenses (incurred to generate donations) and grants (funds paid by one nonprofit to another); ΔTAX_REV is the change in taxable business revenues from the form 990-T; ΔEXEMPT_REV is the change in tax-exempt business revenues from the form 990, which is total revenues less taxable business revenues, donations, and investment revenues; and ΔINVEST_REV is the change in investment revenues from the form 990, which includes interest and dividend revenues. I remove both donations revenue and their related fund-raising expenses from the model because they represent nonbusiness activity. This model is similar to Eldenburg and Soderstrom's (1996) model, except that I use revenues in lieu of volume because volume data are not available. I use first differences to control for organization-specific effects and time-invariant omitted variables.

β_1 is a measure of the change in taxable expenses associated with an additional dollar of taxable revenues. An organization-level estimate of the total variable expenses incurred to generate taxable revenues is the product of β_1 and TAX_REV. I perform similar calculations to estimate the portion of variable expenses allocated to tax-exempt and investment activities:

$$\text{estimated taxable variable expenses} = \beta_1 \times \text{TAX\_REV}_i, \tag{2}$$

$$\text{estimated tax-exempt variable expenses} = \beta_2 \times \text{EXEMPT\_REV}_i, \text{ and} \tag{3}$$

$$\text{estimated investment variable expenses} = \beta_3 \times \text{INVEST\_REV}_i. \tag{4}$$

The sum of equations (2)–(4) is the total organization-level variable expenses incurred by

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[7] I estimated many alternative specifications of this model: logarithmically transforming the data; including squares of the independent variables as additional regressors; using a levels specification; alternately deflating by assets, revenues, and wages; using only those observations where taxable revenues were at least 5 percent of total revenues; estimating separate models for taxable and tax-exempt revenues; using yearly fixed effects; splitting the sample on different dates of receipt of data to mitigate the effects of nonresponse bias; and trimming based on Cook's D of 0.5. Results are generally not altered. In the case of the levels specification, the estimated tax-motivated allocations are higher than those reported in the paper.

a nonprofit. Although this method produces organization-level estimates of variable expenses based on organization-level revenues, the regression coefficients are cross-sectional constants. I attempt to mitigate the effects of constraining the coefficients by estimating equation (1) separately on educational, medical, and charitable nonprofits, and by allowing the coefficients to vary by revenue type (i.e., taxable, tax-exempt, and investment revenues). The next section outlines the method I use to partition the fixed expenses between taxable and tax-exempt activities.

Estimated Fixed Expense Allocations

I estimate fixed expenses by taking the difference between the total expenses the nonprofit reports and my estimate of total variable expenses using equations (2)–(4):

$$\begin{aligned}\text{FIXED}_i = {} & \text{TOTAL\_EXP}_i - (\beta_1 \times \text{TAX\_REV}_i + \beta_2 \times \text{EXEMPT\_REV}_i \\ & + \beta_3 \times \text{INVEST\_REV}_i).\end{aligned} \tag{5}$$

I partition my estimate of firm-level fixed expenses between taxable and tax-exempt activities based on average revenues. The portion allocated to taxable activities is:

$$\text{estimated taxable fixed expenses} = \text{FIXED}_i \times \left(\frac{\text{TAX\_REV}_i}{\text{TAX\_REV}_i + \text{EXEMPT\_REV}_i}\right). \tag{6}$$

By excluding investment revenues from the denominator of equation (6), I assume that the nonprofit incurs no fixed expenses to generate investment revenues. To the extent that a nonprofit does incur this type of fixed expenses, the denominator of equation (6) is too small, causing estimated taxable fixed expenses to be too large. Overestimating the amount of estimated taxable expenses understates my measure of tax-motivated expense allocations, conservatively biasing the results.

Estimated Total Expense Allocations

The estimated total expenses that I allocate to the taxable activities are the sum of equations (2) and (6):

$$\begin{array}{lclcl}\text{estimated} & & \text{estimated taxable} & & \text{estimated taxable} \\ \text{taxable} & = & \text{variable} & + & \text{fixed} \\ \text{expenses} & & \text{expenses} & & \text{expenses.}\end{array} \tag{7}$$

This measure becomes my organization-level expectation of the taxable expenses a nonprofit incurs to earn its taxable revenues. The difference between these estimated expenses and the expenses as reported on the nonprofit's form 990-T is my measure of tax-motivated expense allocations from tax-exempt to taxable activities:

$$\begin{array}{lclcl}\text{tax-motivated} & & \text{reported} & & \text{estimated} \\ \text{expense} & = & \text{taxable} & - & \text{taxable} \\ \text{allocations} & & \text{expenses} & & \text{expenses.}\end{array} \tag{8}$$

One cannot directly map the above model to the reasonable basis standards of the IRS or the courts because those standards indicate that resource use is the appropriate allocation base, whereas my model allocates expenses based on resource output (i.e., revenues). Although my estimates of tax-motivated expense allocations are a plausible heuristic, they may not be interpreted as noncompliance with tax laws.

IV. DATA

Sample Selection

To collect a database of matched sets of tax returns, I requested, in writing, the past three years of forms 990 (publicly available) and 990-T (not publicly available) from the 2,316 nonprofits that filed both forms and that are included in the National Center for Charitable Statistics 1995 database (National Center for Charitable Statistics 1999).[8]

The National Center database includes all nonprofits with total assets of $10 million or more, plus a stratified random sample of smaller organizations, for a total sample of approximately 12,000 nonprofits.[9] The 2,316 nonprofits in the sample account for only 25 percent of the total number of nonprofits that filed a form 990-T in 1995, but more than 85 percent of the total assets and revenues of all nonprofits with taxable income. Of the 2,316 nonprofits, 703 voluntarily supplied matched sets of 990s and 990-Ts. Not all of the nonprofits that sent 990s and 990-Ts provided three years of data. On average, I received 2.6 years of data, yielding a pooled, cross-sectional sample of 1,824 matched sets of forms.

Table 1 describes the distribution of responses to the request for data. The distribution is increasing in time, with 549, 625, and 650 observations from the years 1995, 1996, and 1997, respectively. The highest response rate was for charitable nonprofits (40 percent), followed by educational nonprofits (36 percent), and medical nonprofits (24 percent).

Sample Characteristics

Table 2 reports descriptive statistics of forms 990 and 990-T information for 486 educational nonprofits, 774 medical nonprofits, and 564 charitable nonprofits. All three types report positive total net income on their form 990, suggesting that nonprofits' aggregate activities are profitable. On average, charitable organizations are the most profitable, with a 43 percent profit margin, followed by 23 percent for educational organizations and 7 percent for medical organizations. Median profit margins are significantly smaller for charitable nonprofits (11 percent) and educational nonprofits (13 percent). Donations make up 24 percent of total revenues for educational nonprofits, just over 2 percent for medical nonprofits, and 22 percent for charitable nonprofits. Several of the variables are highly skewed, consistent with other studies (Hines 1999; Cordes and Weisbrod 1998). As a sensitivity check, I re-estimated all models using ranked data with no changes in the study's inferences.

Untabulated results reveal that 85 percent of the medical nonprofits consist of general and specialty hospitals, with the remaining consisting of family planning centers, ambulance services, and blood-supply related nonprofits. Of the educational nonprofits, 80 percent are private universities with the remainder consisting of private high schools, research institutes, and vocational schools. Charitable nonprofits include many different categories, including zoos, theaters, and aquariums, as well as organizations committed to such social causes as providing food, shelter, and clothing.

Only educational nonprofits report mean net profits on their taxable activities, although all nonprofit types report median losses. The sample organization's average loss on taxable

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[8] In addition to requesting the information, my letter stated, "specific information about your organization will not be made public. The study will present only aggregated data, no single organization will be identifiable in any way."

[9] The National Center for Charitable Statistics is a private agency with authority from the IRS to collect and disseminate data on nonprofits.

TABLE 1
Distribution and Frequency of Responses by Nonprofits to a Written Request for Matched Sets of IRS Forms 990 and 990-T

Description	*Educational Nonprofits*	*Medical Nonprofits*	*Charitable Nonprofits*	*Total*
Nonprofits contacted	531	1,239	546	2,316
Unique respondents	189	296	218	703
Response rate	36%	24%	40%	30%
Matched forms received				
1995	160	245	144	549
1996	165	266	194	625
1997	161	263	226	650
Total	486	774	564	1,824
Average number of matched sets of annual returns received per respondent	2.57	2.61	2.59	2.60

This table summarizes the responses to two requests for data mailed to 2,316 nonprofit organizations. I mailed the first request on June 10, 1999 and the second on August 10, 1999. The 2,316 represent all nonprofits that are in the National Center for Charitable Statistics database and that had taxable activities for the 1995 tax year. I requested three years of data from each nonprofit; however, some nonprofits were unable to supply forms from all three years.

activities is just over 1 percent of taxable revenues. In contrast, untabulated analysis based on IRS Statistics of Income files reveals that for the *population* of nonprofits with taxable activities in 1995, average taxable losses were over 33 percent of taxable revenues. This difference between my sample and the underlying population is consistent with less aggressive (or more profitable) nonprofits being willing to supply their otherwise confidential information on their taxable business activities. If the reported taxable activity profitability differences between my sample and the population are due to differences in expense allocation aggressiveness, then my estimates of tax-motivated expense allocations are biased downward. Other than the difference in taxable activity profitability, untabulated logit results suggest that the sample is not jointly different from the population of all nonprofits with taxable activity in 1995 across taxable and tax-exempt revenues, tax-exempt expenses, total assets, and total donations (p-values of 0.149, 0.115, and 0.351 for schools, hospitals, and charities, respectively).

V. TAX-MOTIVATED EXPENSE ALLOCATION TESTS

Variable and Fixed Expenses Estimation

Table 3 reports the results of estimating equation (1). The coefficient estimates for β_1, which represent the increase in expenses for each additional dollar of taxable revenues, are $0.05, $0.11, and $0.11 for educational, medical, and charitable nonprofits (respectively). Although the coefficient estimates for medical and charitable nonprofits are statistically significant at the 5 percent level, the estimate for educational nonprofits is not. The coefficient estimates for β_2, which represent the increase in expenses for each additional dollar of tax-exempt revenues, are $0.38, $0.74, and $0.38 for educational, medical, and charitable

TABLE 2
Descriptive Statistics for a Pooled Sample of 1,824 Matched Sets of Forms 990 and 990-T by Nonprofit Type

	Tax-Exempt Activities (amounts in $ thousands)			*Taxable Activities (amounts in $ thousands)*		
Description	*Mean*	*Median*	*Std. Dev.*	*Mean*	*Median*	*Std. Dev.*
Educational organizations (n = 486)						
Assets	213,519	60,826	714,355			
Revenues	75,649	29,084	248,458	495	125	1,522
Expenses	58,395	23,145	200,680	442	132	1,137
Net income	17,253	3,816	59,055	53	−3	494
Investment income	4,350	1,240	15,176			
Donations	18,233	4,365	67,994			
Fundraising expenses	1,179	621	2,572			
Grants	5,168	1,934	10,713			
Medical organizations (n = 774)						
Assets	134,484	79,282	191,984			
Revenues	110,477	68,995	128,638	608	121	1,358
Expenses	102,335	66,534	120,787	658	126	1,399
Net income	8,142	4,343	13,174	−50	−4	437
Investment income	2,463	1,116	3,697			
Donations	2,522	206	12,310			
Fundraising expenses	199	0	1,828			
Grants	592	0	4,638			
Charitable organizations (n = 564)						
Assets	177,230	27,091	1,658,926			
Revenues	37,288	12,317	174,940	553	101	2,105
Expenses	21,352	10,115	43,052	560	103	2,050
Net income	15,936	1,345	162,651	−7	0	371
Investment income	4,935	461	51,505			
Donations	8,467	2,258	25,696			
Fundraising expenses	731	156	1,809			
Grants	1,643	0	7,284			

Taxable amounts are from the form 990-T, which is an annual tax return that all nonprofits with taxable activity over $1,000 must file. Tax-exempt amounts are from the IRS form 990, which is an annual information return that all nonprofits with gross receipts of more than $25,000, excluding religious organizations, must file. Because the form 990 aggregates both taxable and tax-exempt activities, I take the difference between the aggregate and the taxable amounts to derive the portion attributable solely to tax-exempt activities.

nonprofits (respectively), all of which are statistically significant at the 5 percent level. None of the coefficient estimates for β_3 (the increase in expenses for each additional dollar of investment revenues) are statistically significant. One can interpret the intercepts from equation (1) as the average change in fixed expenses independent of a change in revenues. The point estimates of the intercept are small (ranging from −$0.01 to 0.02) and statistically significant only for charitable nonprofits.

TABLE 3
Regression Estimates Used to Partition Nonprofit Variable Expenses between Taxable and Tax-Exempt Activities

$\Delta TOTAL\_EXP_i = \alpha + \beta_1 \Delta TAX\_REV_i + \beta_2 \Delta EXEMPT\_REV_i + \beta_3 \Delta INVEST\_REV_i + \varepsilon_i$

	α	β_1	β_2	β_3	Adj. R^2	n
Educational nonprofits	−0.01 (−1.58)	0.05 (0.90)	0.38* (7.10)	0.09 (1.72)	0.15	294
Medical nonprofits	0.01 (0.67)	0.11* (3.48)	0.74* (24.2)	0.04 (1.40)	0.59	456
Charitable nonprofits	0.02 (4.89)	0.11* (2.28)	0.38* (7.76)	0.02 (0.43)	0.17	347

* Significant at the 5 percent level.

Variable definitions:

ΔTOTAL_EXP = change in total business expenses (total expenses less fund-raising expenses and grants);
ΔTAX_REV = change in taxable business revenues;
ΔEXEMPT_REV = change in tax-exempt business revenues (total tax-exempt revenues less donations, and investment revenues); and
ΔINVEST_REV = change in investment revenues (interest and dividend income).

All data are from the form 990 except taxable revenues, which are taken from the form 990-T. All models use first-differenced, asset-scaled variables. Influential observations that had a Cook's D statistic greater than 1 were deleted. All t-statistics (in parentheses) are asymptotically corrected (White 1980).

The coefficient estimates for tax-exempt activities (i.e., β_2) significantly exceed those of the taxable activities (i.e., β_1) for all three types of nonprofits at the 5 percent level. There are at least two reasons to expect that the marginal expenses associated with generating an additional dollar of taxable revenues will be less than those of generating an additional dollar of tax-exempt revenues. First, taxable activities may be more marginally profitable because of their role in a nonprofit's mission: to generate additional funds to support the nonprofit's tax-exempt functions. This would lead nonprofits to maximize taxable revenues and minimize taxable expenses in the absence of other objectives. At the same time, nonprofits aim to maximize expenditures on their exempt missions subject to the constraint of current and accumulated revenues (Cordes and Weisbrod 1998; James 1983). These two forces would tend to drive up the marginal profitability of nonprofits' taxable activities relative to their tax-exempt activities. Second, unrelated business revenues can cause the loss of tax-exempt status and evoke a negative public reaction, so nonprofits are likely to require a higher rate of return for taxable activities than for tax-exempt activities.[10]

Comparison of Expected to Actual Expense Allocations

Table 4 compares my estimates of nonprofits' taxable expenses calculated using equations (2)–(8), to the amounts reported on their forms 990-T. The difference represents my

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[10] TIAA-CREF, a retirement fund for educational employees, recently lost its tax-exempt status due to excessive unrelated business income. The president of the American Medical Association resigned under public pressure after entering into a contract to generate unrelated business income by providing the AMA seal of approval on appliances manufactured by Sunbeam Corporation (*Chicago Sun-Times 1997*).

TABLE 4
Estimation of Tax-Motivated Expense Allocations from Tax-Exempt to Taxable Activities (amounts in $ thousands)

$$\textit{estimated taxable variable expenses} = \beta_1 \times TAX\_REV_i$$

$$FIXED_i = TOTAL\_EXP_i - (\beta_1 \times TAX\_REV_i + \beta_2 \times EXEMPT\_REV_i + \beta_3 \times INVEST\_REV_i)$$

$$\textit{estimated taxable fixed expenses} = FIXED_i \times \left(\frac{TAX\_REV_i}{TAX\_REV_i + EXEMPT\_REV_i}\right)$$

$$\textit{estimated taxable expenses} = \textit{estimated taxable variable expenses} + \textit{estimated taxable fixed expenses}$$

$$\textit{tax-motivated expense allocations} = \textit{reported taxable expenses} - \textit{estimated taxable expenses}$$

	Mean	*Median*	*Std. Dev.*
Educational nonprofits (n = 486)			
Estimated taxable expenses	341	67	1,689
Reported taxable expenses	442	132	1,137
Tax-motivated expense allocations	101*	45	949
Tax-motivated expense allocations as a percent of reported expenses	23%	34%	
Medical nonprofits (n = 774)			
Estimated taxable expenses	253	44	604
Reported taxable expenses	658	126	1,399
Tax-motivated expense allocations	405*	86	1,019
Tax-motivated expense allocations as a percent of reported expenses	62%	69%	
Charitable nonprofits (n = 564)			
Estimated taxable expenses	575	106	2,494
Reported taxable expenses	560	103	2,050
Tax-motivated expense allocations	−15	4	1,332
Tax-motivated expense allocations as a percent of reported expenses	0#	0#	

# Reported as zero as the tax-motivated expense allocation estimates are not statistically different from zero.

Variable definitions:

$TOTAL\_EXP_i$ = total business expenses (total expenses less fund-raising expenses and grants);

TAX_REV_i = taxable business revenues;

$EXEMPT\_REV_i$ = tax-exempt business revenues (total tax-exempt revenues less donations, and investment revenues); and

$INVEST\_REV_i$ = investment revenues (interest and dividend income).

The coefficients β_1, β_2, and β_3 are from Table 3. Reported taxable expenses are from Table 2. Tax-motivated expense allocations denoted by * are significantly different from zero at the 5 percent level (two-tailed) based on a Student's t-test of the null hypothesis that reallocated expenses are equal to zero. Inferences using a Wilcoxon signed-rank test are identical.

measure of tax-motivated expense allocations. The mean (median) tax-motivated expense allocations are $0.23 ($0.34), $0.62 ($0.69), and $0.00 ($0.00) per dollar of reported taxable expenses for educational, medical, and charitable nonprofits (respectively). The finding that medical nonprofits reallocate the largest proportion of expenses from tax-exempt to taxable activities is consistent with prior evidence that medical nonprofits behave more opportunistically than do educational and charitable nonprofits (Steinberg 1986). The finding that charitable nonprofits do not significantly reallocate expenses from tax-exempt to taxable activities suggests that the effects of the unrelated business income tax depends on the nonprofits' primary purpose.

Some of the tax-motivated allocation percentages appear to be relatively large (e.g., I estimate that 62 percent of medical nonprofits' reported taxable expenses are the result of reallocations from their tax-exempt activities). In absolute terms, however, reallocating only 0.4 percent of tax-exempt expenses to taxable activities will produce a 62 percent tax-motivated expense allocation estimate for medical nonprofits.

Table 2 shows that medical nonprofits in the sample report average taxable net losses of $50 thousand. Adding back my estimate of tax-motivated allocations of $405 thousand (Table 4) suggests that medical nonprofits should report a net profit on taxable activities of $355 thousand ($405 − $50). A similar calculation for educational nonprofits suggests that, rather than reporting an average taxable net income of $53 thousand, average taxable net income should be $154 thousand ($101 + $53).

Of course, the accuracy of my estimated tax-motivated expense allocations is a function of the model, its related assumptions, and independent variable measurement error issues. Although plausible, the estimated tax-motivated allocations may not capture the true underlying (and inherently unmeasurable) allocations. However, to the extent that the estimates produced by my revenue-based models are good approximations for the estimates that would be produced by a resource-based model, the results are useful for calibrating the extent to which tax-motivated allocations contribute to the reported losses on nonprofits' taxable activities.

Differential Tax-Motivated Allocations across Expense Categories

Prior tax compliance research (Clotfelter 1983) suggests that aggressive tax behavior is more likely to occur in poorly defined expense categories (i.e., other expenses) and is less likely to occur in accounts that are subject to third-party reporting to the IRS. To test for this effect, I partition all expenses into one of three categories: (1) reported by third-parties and well-defined, such as officers' compensation and interest expense, (2) not reported by third parties but well-defined, such as depreciation expense, and (3) not reported by third parties and not well-defined, such as "other" expenses. I consider an expense to be well-defined if has a separate line item description on both the form 990 and 990-T. I expect that nonprofits are most likely to reallocate expenses from their tax-exempt to their taxable activities in the "not third-party reported and not well-defined" category, and least likely to reallocate expenses in the "third-party reported and well-defined" category.

Ideally, I would conduct this analysis by estimating three expense-category-specific coefficients using equation (1). Although it is possible to estimate three separate coefficients in equation (1), I could not use these coefficients to partition expenses between taxable and tax-exempt activities because equation (2) multiplies the coefficients by taxable revenues, which I cannot partition across the three expense categories. (Multiplying the three separate coefficients by total taxable revenues would overestimate the expenses allocable to the taxable activity because total revenues would be multiplied three times.) Therefore I must use the aggregate coefficients for this test.[11]

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[11] As a sensitivity test, I estimated three coefficients (one for each expense category) in equation (1) and tested for their differences. Untabulated results reveal that the coefficient estimates for the "not-reported-and-not-well-defined" and the "not-reported-but-well-defined" categories are not significantly different at the 5 percent level, providing some comfort that the results are unlikely to be sensitive to using aggregate coefficient estimates.

The results in Table 5 are consistent with expectations. For all nonprofit types, I do not find significant tax-motivated expense allocations in the "third-party reported and well-defined" category, suggesting that nonprofits avoid reallocating expenses from tax-exempt to taxable activities that are highly visible to the IRS. I find the most significant tax-motivated expense allocations in the "not third-party reported and not well-defined" expense category for educational and medical nonprofits, consistent with the notion that nonprofits reallocate expenses where they are least likely to draw attention. Finally, I find significant tax-motivated expense allocations in the "not third-party reported but well-defined" expense category for medical nonprofits only. Untabulated results reveal that the differences between each of the three expense categories are statistically significant at the 5 percent level for medical and educational nonprofits.

VI. CONCLUSIONS

This study examines nonprofit organizations' expense allocation behavior in response to the tax on their unrelated business income. Although nonprofits are generally exempt from income taxation, they are taxed similarly to for-profit corporations on the portion of

TABLE 5
Estimation of Tax-Motivated Expense Allocations by Expense Category
(amounts in $ thousands)

Description	*Mean*	*Median*	*Mean as a Percent of Total Expenses*
Educational nonprofits			
Third-party reporting and well-defined	1	0	0
No third-party reporting but well-defined	20	5	0
No third-party reporting and not well-defined	89*	33	31
Medical nonprofits			
Third-party reporting and well-defined	3	0	0
No third-party reporting but well-defined	153*	18	76
No third-party reporting and not well-defined	324*	63	73
Charitable nonprofits			
Third-party reporting and well-defined	−1	0	0
No third-party reporting but well-defined	−10	0	0
No third-party reporting and not well-defined	−49	2	0

I estimated these amounts using the aggregate coefficient estimates from Table 3. Tax-motivated expense allocations denoted by * are significantly different from zero at the 5 percent level (two-tailed) based on a Student's t-test of the null hypothesis that tax-motivated allocated expenses are equal to zero. Inferences using a Wilcoxon signed-rank test are identical. Differences across the three expense categories are statistically different at the 5 percent level for educational and medical nonprofits.

For this analysis, expenses are separated into three categories based on the extent of third-party reporting to the IRS and whether the expense is well-defined with a separate line item description on the form 990 and 990-T. The first category is "Third-party reporting and well-defined" and includes interest, tax, and officers' compensation expenses, all of which are reported to the IRS by third parties and have separate line items. The second category is "No third-party reporting but well-defined" and includes expenses that have separate line items but are not subject to third-party reporting to the IRS. The third category is "No third-party reporting and not well-defined" and includes expenses in the "other" account.

their activities not closely related to their exempt purpose. Because nonprofits can have both taxable and tax-exempt activities, they have an incentive to shift expenses from tax-exempt activities to taxable activities to reduce overall tax liabilities. In the aggregate, nonprofits report losses on their taxable activities of more than 30 percent of revenues. Using a database of more than 1,800 confidential income tax returns, I develop a model of expected expenses for taxable activities and then compare these estimates to those actually reported on nonprofits' tax returns to assess the extent to which the reported losses are likely due to tax-motivated expense allocations. Of course, the accuracy of these estimates is a function of the model, its related assumptions, and independent variable measurement error issues. Although plausible, my estimates capture the true underlying (and inherently unmeasurable) tax-motivated allocations with error.

The study represents the first large-sample, organization-level empirical analysis of the unrelated business income tax, one of only four types of income taxes in the United States. The results suggest that medical (educational) nonprofits in the sample responded to tax incentives by reallocating an average of $400 ($100) thousand of expenses from their tax-exempt activities to their taxable activities. I did not find evidence of tax-motivated expense allocations by charitable nonprofits. Most of the tax-motivated expense allocations occur in categories that are not subject to third-party reporting to the IRS and are poorly defined, such as "other" expenses. On average, the sample's taxable activities are more profitable than the population's. If the profitability differences between my sample and the related population are due to differences in expense allocation aggressiveness, then my estimates of tax-motivated expense allocations are likely biased downward.

These results suggest that the recurring aggregate losses nonprofits report on their unrelated taxable activities are likely at least partially attributable to tax-motivated expense allocations. Given the ambiguity in applicable tax law, it is not clear whether these tax-motivated allocations are completely compliant. In any event, to the extent that nonprofits allocate expenses from their tax-exempt to their taxable activities to avoid the unrelated business income tax, the tax as currently structured fails to fully achieve its goals of preventing unfair competition and raising revenues.

Although these results provide an initial approximation of the effect of tax incentives on nonprofit behavior, future research may refine the analysis by using facility use data, examining why educational and medical nonprofits respond to tax incentives while charitable nonprofits do not, and expanding the analysis beyond 501(c)(3) organizations.

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The Effect of Missing a Quarterly Earnings Benchmark on the CEO's Annual Bonus

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ABSTRACT: We investigate the effects of missing quarterly earnings benchmarks on the CEO's annual bonus. After controlling for the general pay-for-performance relation, we find a significant incremental adverse effect on CEO annual cash bonuses when the firm's quarterly earnings fall short of the consensus analyst forecast or the earnings for the same quarter of the prior year, for at least two quarters during the year. However, we find that the relation between the bonus and the number of loss quarters is not significant. Our results suggest that CEO bonus payments provide CEOs with economic incentives to meet quarterly analyst earnings forecasts and earnings from the same quarter of the prior year.

Keywords: *executive compensation; quarterly earnings benchmark; earnings management.*

I. INTRODUCTION

Recent empirical evidence suggests that managers adjust earnings to meet various benchmarks (e.g., Burgstahler and Dichev 1997; DeFond and Park 1997; Brown forthcoming; Degeorge et al. 1999; Myers and Skinner 1999). Specifically, these studies examine earnings patterns around various benchmarks, and conclude that managers try to avoid reporting losses, decreases in earnings from the prior year, and earnings that

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fall short of the consensus analyst forecast.[1] However, there is little evidence on the precise incentives that lead CEOs to manage earnings to meet or beat these benchmarks. Such incentives could include job security, a possible decline in the value of the CEO's equity holdings, or compensation arrangements.

In this paper we focus on CEO compensation and examine whether the CEO's bonus is lower when the firm misses quarterly earnings benchmarks (the quarterly analyst forecast, the same quarter of the prior year, and zero). Our analysis controls for the general pay-for-performance relation, because prior research has documented that CEO bonus payments are positively associated with both accounting and stock price measures of firm performance (e.g., Janakiraman et al. 1992; Clinch and Magliolo 1993; Dechow et al. 1994; Baber et al. 1998; Gaver and Gaver 1998; Baber et al. 1999). If the failure to achieve a quarterly earnings benchmark is also an important aspect of accounting performance, then missing a benchmark is likely to have an *incremental* adverse effect on the CEO's compensation beyond a normal penalty for poor performance. As an example, the December 7, 2000 issue of the *Wall Street Journal* reports, "Carly Fiorina, its [Hewlett-Packard Co.'s] president, chairman and chief executive officer, told analysts in a conference call that she and other senior executives will receive no executive bonuses for the second half of the fiscal year ended Oct. 31, as a result of missing analysts' expectations for the fourth quarter (Dow Jones Newswires 2000)."

Because details of CEO compensation contracts are not publicly available, we cannot document a direct contractual link between missing an earnings benchmark and a CEO's bonus payment. However, our discussions with a leading compensation consultant suggest that the effect of missing a quarterly earnings benchmark on a CEO's bonus is likely to result from the compensation committees' exercise of their discretion in the allocation of the bonus pool, as opposed to the benchmark's being an explicit component of the plan.

For our empirical analysis we use a total of 3,651 firm-year observations drawn from the intersection of the ExecuComp, First Call, Compustat, and CRSP databases. We examine the empirical relations between the change in the CEO's bonus (scaled by prior year's salary) and dummy variables representing the frequency with which the firm missed quarterly earnings benchmarks.

To control for overall performance, our regression analysis includes the change in ROA and the firm's stock return, and sensitivity analyses also control for analysts' annual earnings forecast error. In addition, prior research documents that the pay-performance sensitivity is attenuated when performance is poor (e.g., Gaver and Gaver [1998] show that CEOs are more likely to reap rewards for good performance than to be penalized for poor performance), and firms missing a benchmark are likely to be poor performers. Thus, we also control for the interaction between the number of quarters that the firm missed the benchmark and the change in ROA, to ensure that our dummy variables for missing the earnings benchmarks do not spuriously reflect the asymmetric pay-performance sensitivity for poor performers. Given these controls, the estimated coefficients on the earnings benchmark dummy variables indicate whether CEO bonuses suffer an incremental "penalty" for falling short of the benchmark, beyond the "normal" penalty for poor performance.

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[1] The results for the analyst forecast benchmark are consistent with the concerns expressed by Securities and Exchange Chairman Arthur Levitt (1998) that firms manipulate earnings to achieve consensus earnings estimates.

With the analyst forecast and the earnings from the same-quarter-of-the-prior-year benchmarks, we find a significantly negative incremental effect on CEO bonuses when the firm misses the benchmark more than once during the year. We also find weak evidence that missing the benchmark just once during the year also has a negative incremental effect on CEO bonuses, although this finding is not robust to alternative specifications. In contrast, we do not find consistent evidence of a significantly negative relation between CEO compensation and the number of quarters during the year that the firm reports net losses (the zero earnings benchmark). Our sample firms report a quarterly loss in only 6 percent of the sample quarters, suggesting that our sample has an abnormally high proportion of successful firms; so avoiding a loss seems to be a relatively low benchmark.

Overall, our evidence indicates that CEO bonuses provide managers with an incentive to meet analyst earnings forecasts and the earnings for the same quarter of the prior year. Such evidence contributes to the earnings management literature by documenting an incentive for CEOs to manage earnings in order to meet benchmarks. In addition, our results suggest that the strength of those incentives depends on whether the firm has missed a quarterly benchmark earlier during the year. Our evidence also provides additional insight into the pay-for-performance relation. For example, Dechow et al. (1994) and Gaver and Gaver (1998) provide evidence that CEO compensation is at least partially protected from unfavorable events. However, our evidence suggests that this protection is not complete in that CEOs are penalized for failing to meet certain earnings benchmarks.

Because our sample selection criteria led to a preponderance of larger, more successful firms, our inferences may not generalize to populations of smaller, or less successful firms. On the other hand, our sample is based upon a relatively large sample of 3,651 firm-year observations that comprise the intersection of populations often used by accounting researchers.

The next section summarizes the related literature on earnings management and develops the study's hypotheses. In Section III, we discuss our sample selection procedure and research design. We discuss the results of our tests in Section IV and summarize our findings and conclusions in Section V.

II. HYPOTHESIS DEVELOPMENT

Recent research has documented unusual earnings patterns around various quarterly or annual benchmarks. Burgstahler and Dichev (1997) document an unusually low (high) number of reported annual earnings fall just below (above) zero and the earnings for the prior year. Burgstahler and Eames (1999) find a similar pattern for annual earnings relative to the analyst forecast benchmark. Burgstahler (1997) (for zero earnings or the earnings for the same quarter of the previous year) and Degeorge et al. (1999) (for all three benchmarks) document similar patterns for quarterly earnings. In a related paper, Brown (forthcoming) shows that loss surprises (relative to analysts' forecasts) are usually negative, and often extreme, whereas profit surprises are usually positive, and often small. This evidence suggests that profitable firms adjust earnings to meet the analysts' forecasts.

The authors of these papers suggest that their evidence is consistent with earnings management in order to meet a given earnings benchmark. However, little evidence is available on specific consequences to management that result from failing to meet such benchmarks.

We investigate the extent to which CEO compensation suffers from an incremental penalty when the firm misses an earnings benchmark. The compensation committee may exact a penalty if it views missing an earnings benchmark as a signal of poor performance.[2] On the other hand, beating the benchmark could constitute a public, and often well-publicized, signal that allows the firm's compensation committee to justify higher compensation levels to outside shareholders and thereby overcome the political constraints on CEO compensation suggested by Jensen and Murphy (1990). Given that several studies, including Barth et al. (1999), DeFond and Park (2000), and Kinney et al. (2000), document negative market reactions to missed earnings benchmarks, directors concerned about a negative stock price reaction may also reward the manager for meeting earnings benchmarks.

Finally, the board may want to encourage managers to provide analysts with more accurate earnings-related information. Penalizing the manager for failing to achieve the forecast could motivate the manager to truthfully communicate the earnings news.[3] In other words, by exacting a penalty on the CEO (in the form of a lower bonus) when the firm misses the consensus forecasts, boards encourage CEOs to truthfully reveal earnings-related information so analysts can develop more credible earnings forecasts. For example, Matsumoto (2000) presents evidence consistent with managers attempting to guide analyst forecasts.

Despite the resources at the CEO's disposal, failure to meet the benchmark is not uncommon. We find that 34 percent of our sample observations report earnings below the consensus analyst forecast even though the CEO can make production and operating decisions, adjust accounting estimates, or even influence the earnings forecast. This suggests that the benchmarks are fairly difficult to achieve and that there are substantial limits to the CEO's ability to manage earnings (or the forecast) to meet that forecast.

In any case, it is an empirical question whether the board reduces CEO pay when the firm misses a quarterly earnings benchmark. While compensation committees determine CEO pay on an annual basis, firms typically report earnings on a quarterly basis. Given the documented positive relation between annual performance and annual CEO compensation, we investigate whether CEOs suffer an incremental bonus penalty when the firm misses a quarterly earnings benchmark, after controlling for annual performance.

Detailed descriptions of CEO compensation contracts are generally not publicly available, so we cannot use the actual parameters of executive compensation contracts. As a result, empirical studies of executive compensation, (e.g., Defeo et al. 1989; Clinch and Magliolo 1993; Dechow et al. 1994; Baber et al. 1998; Gaver and Gaver 1998), relate the amount of the bonus paid to observable measures of performance. In our case, we assess the relation between the amount of the bonus and the number of quarters in which the firm fails to meet a quarterly earnings benchmark. Evidence that missing the benchmark is associated with lower bonuses would be consistent with explicit contractual penalties for missing the benchmark, or with informal arrangements whereby the compensation committee considers performance relative to the benchmark to justify the magnitude and allocation of the bonus pool.

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[2] Discussing the removal of Dial Corporation's CEO in an August 2000 *Wall Street Journal* article, Lipin and Mathews report, "The move came after the board heard the company would have to warn investors for the third time in a row that quarterly earnings would be below analysts' estimates." This anecdote is consistent with the board's viewing a missed earnings benchmark as a signal of poor performance. The removal of a CEO can be considered an extreme form of a compensation penalty. However, a direct investigation of CEO turnover is beyond the scope of our study.

[3] The credibility of corporate disclosures has received increasing attention. For example, in the June 21, 1999 issue of *Fortune*, Sellers lists several CEOs who have "major credibility problems with investors."

Hypothesis: *Ceteris paribus*, the change in the CEO's bonus is lower when a firm misses a quarterly earnings benchmark.

The *ceterus paribus* condition means that the hypothesis predicts a negative incremental effect of missing the benchmark after controlling for the "normal" effect of performance on pay.

III. RESEARCH DESIGN

Sample Selection

Our sample is based on the intersection of the ExecuComp, First Call, Compustat, and CRSP databases from 1993 to 1997. We obtain our data on CEO compensation and equity holdings from ExecuComp. We use First Call to obtain the quarterly earnings forecasts. Compustat and CRSP provide firm-specific financial information for our control variables. In addition, we require that the CEO hold that position for the full fiscal year; we delete partial-year CEOs. We also delete acting CEOs, co-CEOs, CEOs of subsidiaries, CEOs of firms with zero or negative stockholders equity, and CEOs of firms that changed their fiscal year-ends in the current or previous year. We require each firm to have quarterly earnings data for all four quarters of the given year. Finally, to reduce the effects of extreme observations, we drop firms in the top and bottom 1 percent of the distributions of the change in annual ROA and changes in our compensation measures, as defined below.

This procedure yields a total of 3,651 firm-year observations. Panel A of Table 1 presents the number of observations in each year. First Call's coverage expanded over our sample period, so the observations cluster in the more recent years. Our sample includes a total of 1,324 different firms and only 129 executives (10 percent) have the maximum number of five observations in the sample.

Empirical Test of the Hypothesis

We hypothesize an association between missing a quarterly earnings benchmark and a reduction in the CEO's cash bonus. To test this hypothesis, for each benchmark we regress the change in the CEO cash bonus on the number of quarters during the year that the firm failed to meet the benchmark level of earnings. Because the effect on compensation may not be linear in the number of quarters that the firm misses its benchmark, we use separate variables for the number of quarters during the year that the firm misses the earnings benchmark (once, twice, etc).

The regression includes variables to control for both accounting and market performance. These controls are critical to separate the incremental effect of a missed earnings benchmark from overall performance. Consistent with past research (for example, Janakiraman et al. 1992), we include the change in ROA from period $t - 1$ to t as our control for accounting-based performance, and the firm's annual stock return in year t as our control for market performance.

In an effort to better disentangle the effects on compensation of missing the quarterly earnings benchmarks from the effects on compensation of poor performance, our controls allow for asymmetric pay-performance sensitivity (i.e., a change in the slope in the compensation/earnings relation). Because prior research suggests the sensitivity of pay to performance is asymmetric (CEOs are more likely to receive a larger bonus for good performance than to be penalized for poor performance), we include an interaction term that allows the slope coefficient on the change in the annual ROA to differ based on the number of missed benchmarks.

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TABLE 1
Sample Descriptive Statistics

Panel A: Number of Observations by Year

Year	*Number of Observations*	*Percentage of Total Sample*
1993	236	6.5
1994	633	17.3
1995	862	23.6
1996	967	26.5
1997	953	26.1
Total	3,651	100.0

Panel B: Number of Quarters for which Firms Miss Their Benchmarks

	Firms with Positive Quarterly Earnings (% of total number of observations)		
	Positive Change[a]	*Negative Change*	*Total*
Meet or Exceed Forecast[b]	8,217 (56.3%)	1,205 (8.3%)	9,422 (64.6%)
Miss Forecast	2,361 (16.2%)	1,973 (13.5%)	4,334 (29.7%)
Total	10,578 (72.5%)	3,178 (21.8%)	13,756 (94.3%)

	Firms with Negative Quarterly Earnings (% of total number of observations)		
	Positive Change	*Negative Change*	*Total*
Meet or Exceed Forecast	152 (1.0%)	124 (0.9%)	276 (1.9%)
Miss Forecast	131 (0.9%)	441 (3.0%)	572 (3.9%)
Total	283 (1.9%)	565 (3.9%)	848 (5.8%)

(*Continued on next page*)

TABLE 1 (Continued)

Panel C: Comparison to Other Samples

	Our Sample	*First Call*	*Compustat*[c]
% Miss forecast	34%	38%	NA
% Negative change	26%	32%	38%
% Quarterly loss	6%	16%	31%
Median ROA	10%	9%	6%
Median Return	17%	17%	10%

Panel D: Distributions of Continuous Variables

Variable	*Mean*	*Standard Deviation*	*First Quartile*	*Median*	*Third Quartile*
ΔCOMP	0.122	0.485	−0.061	0.067	0.312
RET	0.218	0.409	−0.021	0.174	0.404
ΔROA	−0.001	0.050	−0.013	0.001	0.015

Panel E: Frequency Distribution of Missed Benchmark Dummy Variables

	Number of Observations (%) *Benchmark*		
Number of Quarters Missed	*Analyst Forecast*	*Prior Year Earnings*	*Zero*
0	1,268 (34.7%)	1,887 (51.7%)	3,198 (87.6%)
1	903 (24.7%)	653 (17.9%)	235 (6.4%)
2	720 (19.7%)	488 (13.4%)	101 (2.8%)
3	477 (13.1%)	378 (10.3%)	57 (1.6%)
4	283 (7.8%)	245 (6.7%)	60 (1.6%)

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[a] Positive change refers to an increase in earnings over the same quarter of the previous year.

[b] We obtain actual earnings and the latest consensus earnings forecast from First Call.

[c] For Compustat earnings we use quarterly data item #19 (primary earnings per share excluding extraordinary items).

ΔCOMP = change in the CEO's annual bonus scaled by salary at t − 1;
RET = annual stock return; and
ΔROA = change in ROA, defined as the ratio of annual earnings before interest and taxes to average total assets.

We estimate the following regressions to test our hypothesis:

$$\Delta COMP_{it} = \alpha_0 + \alpha_1 NEGFE1_{it} + \alpha_2 NEGFE2_{it} + \alpha_3 NEGFE3_{it} + \alpha_4 NEGFE4_{it} + \alpha_5 \Delta ROA_{it} + \alpha_6 RET_{it} + \alpha_7 NEGFE1_{it} \times \Delta ROA_{it} + \alpha_8 NEGFE2_{it} \times \Delta ROA_{it} + \alpha_9 NEGFE3_{it} \times \Delta ROA_{it} + \alpha_{10} NEGFE4_{it} \times \Delta ROA_{it} + e_1; \quad (1)$$

$$\Delta COMP_{it} = \beta_0 + \beta_1 DECREASE1_{it} + \beta_2 DECREASE2_{it} + \beta_3 DECREASE3_{it} + \beta_4 DECREASE4_{it} + \beta_5 \Delta ROA_{it} + \beta_6 RET_{it} + \beta_7 DECREASE1_{it} \times \Delta ROA_{it} + \beta_8 DECREASE2_{it} \times \Delta ROA_{it} + \beta_9 DECREASE3_{it} \times \Delta ROA_{it} + \beta_{10} DECREASE4_{it} \times \Delta ROA_{it} + e_2; \quad (2)$$

$$\Delta COMP_{it} = \gamma_0 + \gamma_1 LOSS1_{it} + \gamma_2 LOSS2_{it} + \gamma_3 LOSS3_{it} + \gamma_4 LOSS4_{it} + \gamma_5 \Delta ROA_{it} + \gamma_6 RET_{it} + \gamma_7 LOSS1_{it} \times \Delta ROA_{it} + \gamma_8 LOSS2_{it} \times \Delta ROA_{it} + \gamma_9 LOSS3_{it} \times \Delta ROA_{it} + \gamma_{10} LOSS4_{it} \times \Delta ROA_{it} + e_3; \quad (3)$$

where for each firm i and year t:

ΔCOMP = change in CEO's bonus deflated by prior year salary;[4]
NEGFE(J) = 1 if earnings were below the consensus analyst forecast (negative forecast error) for exactly J quarters during the year and 0 otherwise;
DECREASE(J) = 1 if earnings were below earnings for the same quarter for the previous year (negative change in quarterly earnings) for exactly J quarters during the year, and 0 otherwise;
LOSS(J) = 1 if earnings were below zero (negative earnings) for exactly J quarters during the year and 0 otherwise;
ΔROA = the annual change in ROA, defined as the ratio of earnings before interest and taxes (Compustat annual item #178) to average total assets; and
RET = annual return measured as the monthly stock returns compounded over the 12-month fiscal year.

Our main tests estimate equations (1)–(3) on a pooled cross-sectional, time-series basis. Our hypothesis predicts that even after controlling for the general pay-performance relation, there will still be an incremental penalty for failing to meet a benchmark, i.e., $\{\alpha_1, \alpha_2, \alpha_3$, and $\alpha_4 < 0$; $\beta_1, \beta_2, \beta_3$, and $\beta_4 < 0$; $\gamma_1, \gamma_2, \gamma_3$, and $\gamma_4 < 0\}$.

Gaver and Gaver (1998) provide evidence of a stronger pay-for-performance relation for earnings than for losses. While their evidence relates explicitly to only the zero earnings benchmark, a general attenuation of the pay-for-performance relation would result in an interaction effect for missing any of the benchmarks. In short, while γ_7 to $\gamma_{10} < 0$ would be consistent with the Gaver and Gaver (1998) findings, α_7 to $\alpha_{10} < 0$ and β_7 to $\beta_{10} < 0$ would also be consistent with a lower pay-performance sensitivity when performance is poor (i.e., below the consensus analyst forecast and below earnings of the same quarter of the prior year, respectively).

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[4] We do not include the change in salary because the salary level in year t is generally set at the beginning of the period, and thus is unlikely to be affected by performance in year t. The salary deflator provides an intuitive measure of the relative magnitude of the change in bonus, and is consistent with prior research (Baber et al. 1996; Baber et al. 1998; Baber et al. 1999).

In supplementary analysis, we also examine the incremental effect of missing each of the three benchmarks. Because of the small number of observations for which a firm misses only one benchmark, we are unable to use the Degeorge et al. (1999) procedure of examining each benchmark, conditional on meeting the other two benchmarks. For example, our sample includes only 152 (1 percent) quarters in which a firm meets the earnings forecast and reports an earnings increase but reports a loss. Therefore, we examine the incremental compensation effect of missing each benchmark by including all three benchmarks in the following regression:

$$\begin{aligned}\Delta COMP_{it} = {} & \delta_0 + \delta_1 NEGFE1_{it} + \delta_2 NEGFE2_{it} + \delta_3 NEGFE3_{it} + \delta_4 NEGFE4_{it} \\ & + \delta_5 DECREASE1_{it} + \delta_6 DECREASE2_{it} + \delta_7 DECREASE3_{it} \\ & + \delta_8 DECREASE4_{it} + \delta_9 LOSS1_{it} + \delta_{10} LOSS2_{it} + \delta_{11} LOSS3_{it} \\ & + \delta_{12} LOSS4_{it} + \delta_{13} \Delta ROA_{it} + \delta_{14} RET_{it} + \delta_{15} NEGFE1_{it} \\ & \times \Delta ROA_{it} + \delta_{16} NEGFE2_{it} \times \Delta ROA_{it} + \delta_{17} NEGFE3_{it} \\ & \times \Delta ROA_{it} + \delta_{18} NEGFE4_{it} \times \Delta ROA_{it} + \delta_{19} DECREASE1_{it} \\ & \times \Delta ROA_{it} + \delta_{20} DECREASE2_{it} \times \Delta ROA_{it} + \delta_{21} DECREASE3_{it} \\ & \times \Delta ROA_{it} + \delta_{22} DECREASE4_{it} \times \Delta ROA_{it} + \delta_{23} LOSS1_{it} \\ & \times \Delta ROA_{it} + \delta_{24} LOSS2_{it} \times \Delta ROA_{it} + \delta_{25} LOSS3_{it} \times \Delta ROA_{it} + \delta_{26} LOSS4_{it} \\ & \times \Delta ROA_{it} + e_4. \end{aligned} \tag{4}$$

If missing a benchmark has an incremental effect on compensation, then the coefficients for the dummy variables for missing benchmarks, δ_1 through δ_{12}, will be negative. However, given the overlap among benchmarks, if one benchmark subsumes the effects of the others, we may observe a negative coefficient for the dominant benchmark and insignificant signs on the other benchmarks.

We use the consensus (mean) analyst forecast closest to the earnings announcement date and actual earnings from First Call to measure forecast errors and earnings changes. First Call updates its earnings forecasts on a real-time basis whenever analysts revise their forecasts. In addition, First Call reports "actual" and forecast earnings from continuing operations, which exclude unusual and extraordinary items. Wall Street analysts typically rely on this measure because it reflects the persistent component of earnings (McDermott 1999; Brown and Sivakumar 2000). Furthermore, Baber et al. (1998) find that the sensitivity of compensation to earnings increases with earnings persistence. Thus, we base our earnings benchmarks on earnings from continuing operations.

Panel B of Table 1 reports the distribution of quarters in which firms met, or missed, each benchmark. Forecasted earnings are the most frequently missed benchmark (4,906 quarters, or 34 percent), followed by prior-year quarterly earnings (3,743 quarters, or 26 percent) and zero, (848 quarters, or 6 percent).[5] The data further suggest that a firm that reports declining earnings is likely to miss the earnings forecast. In addition, firms that report losses often miss the other two benchmarks. On the other hand, for firms reporting positive earnings, those that miss forecasted earnings are more likely to report earnings increases than earnings decreases.

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[5] Of our firm-quarters, 18.8 percent have zero forecast errors. This is close to the overall frequency of 17.0 percent for all First Call firms over our sample period. Of our sample observations, 47.6 percent beat analysts' forecasts, which is slightly higher than the overall percentage (44.4) for all First Call firms over our sample period. On average, most recent analyst consensus forecasts for our sample firms and overall First Call firms reveal a pessimistic bias. This is consistent with Brown (forthcoming), who observes a pessimistic bias for profit firms covered by I/B/E/S analysts.

The ExecuComp and First Call databases lead to a sample of relatively successful firms. Panel C of Table 1 compares the frequency of missed benchmarks in our sample to the frequency observed in the full First Call database over the sample period. The firms in our sample miss benchmarks less frequently than the full First Call and Compustat populations. For example, only 6 percent of our firm-quarters report losses, as opposed to 16 percent for First Call firms and 31 percent for all Compustat firms. In addition, our sample firms' accounting and market returns are similar to those for the First Call population, but they are well above those for the Compustat sample.[6]

Panel D of Table 1 provides descriptive data on the distributions of the variables used in the main tests. The median change in the bonus represents 6.7 percent of salary, which is higher than the 0.0 percent reported by Baber et al. (1999). This difference likely stems from both a difference in sample and a difference in time period, as Baber et al. (1999) only include data for 1992 and 1993. For 1993, the median change in bonus as a percentage of salary for our sample is 4.8. While this is closer to the figure reported by Baber et al. (1999), it is still substantially higher than that figure. In addition, our sample's 17.4 percent median stock return exceeds the median returns reported by Baber et al. (1999) of 12 percent in 1992 and 9 percent in 1993. These differences are consistent with the contention that the ExecuComp and First Call data sources we used to generate our sample tend to track relatively more successful firms, and that our sample period reflects a strong economy.

In Panel E of Table 1 we present the frequency distributions for our earnings benchmark variables. For each of the three benchmarks, firms are most likely to meet the benchmark in all four quarters, or to miss the benchmark in just one quarter. Nonetheless, with the exception of the zero earnings benchmark, a nontrivial proportion of firms miss their quarterly benchmarks more than once during a year. Specifically, 41 percent of the firm-quarter earnings missed the analyst forecast and 30 percent missed the same quarter of the prior-year benchmark more than once during the year.

IV. RESULTS

In this section we report the results of tests investigating the impact on CEO compensation of missing a quarterly earnings benchmark. We first report the main results for three benchmarks (forecasted earnings, earnings for the same quarter of the prior year, and zero earnings) and then we discuss sensitivity tests and alternative specifications.

Primary Results

The study's primary results appear in Table 2.[7] The first column examines the effect on the CEO's bonus of missing the consensus analyst forecast. Consistent with the hypothesis, each dummy variable corresponding to the number of quarters in which the firm reported earnings below the consensus analyst forecast is significantly negative. In addition, the bonus penalty appears to increase when the firm misses the forecast or prior-year

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[6] We are not aware of an earnings definition on Compustat that exactly matches the First Call definition. Since Brown and Sivakumar (2000) use quarterly data item #19 (primary earnings per share excluding extraordinary items) as a measure of GAAP quarterly earnings to compare with the earnings per share reported in the I/B/E/S database, we also use data item #19 for Compustat earnings.

[7] We compute condition numbers for all the regressions reported in Table 2. In no case does any condition number exceed 10. Thus, there is no evidence of serious multicollinearity problems (see Belsley et al. 1980).

TABLE 2
The Effects of Missing Quarterly Earnings Benchmarks on the Change in CEO Bonus

Regression Models:

$$\Delta COMP_{it} = \alpha_0 + \alpha_1 NEGFE1_{it} + \alpha_2 NEGFE2_{it} + \alpha_3 NEGFE3_{it} + \alpha_4 NEGFE4_{it} + \alpha_5 \Delta ROA_{it} + \alpha_6 RET_{it} + \alpha_7 NEGFE1_{it} \times \Delta ROA_{it} + \alpha_8 NEGFE2_{it} \times \Delta ROA_{it} + \alpha_9 NEGFE3_{it} \times \Delta ROA_{it} + \alpha_{10} NEGFE4_{it} \times \Delta ROA_{it} + e_1$$

$$\Delta COMP_{it} = \beta_0 + \beta_1 DECREASE1_{it} + \beta_2 DECREASE2_{it} + \beta_3 DECREASE3_{it} + \beta_4 DECREASE4_{it} + \beta_5 \Delta ROA_{it} + \beta_6 RET_{it} + \beta_7 DERCREASE1_{it} \times \Delta ROA_{it} + \beta_8 DECREASE2_{it} \times \Delta ROA_{it} + \beta_9 DECREASE3_{it} \times \Delta ROA_{it} + \beta_{10} DECREASE4_{it} \times \Delta ROA_{it} + e_2$$

$$\Delta COMP_{it} = \gamma_0 + \gamma_1 LOSS1_{it} + \gamma_2 LOSS2_{it} + \gamma_3 LOSS3_{it} + \gamma_4 LOSS4_{it} + \gamma_5 \Delta ROA_{it} + \gamma_6 RET_{it} + \gamma_7 LOSS1_{it} \times \Delta ROA_{it} + \gamma_8 LOSS2_{it} \times \Delta ROA_{it} + \gamma_9 LOSS3_{it} \times \Delta ROA_{it} + \gamma_{10} LOSS4_{it} \times \Delta ROA_{it} + e_3$$

$$\Delta COMP_{it} = \delta_0 + \delta_1 NEGFE1_{it} + \delta_2 NEGFE2_{it} + \delta_3 NEGFE3_{it} + \delta_4 NEGFE4_{it} + \delta_5 LOSS1_{it} + \delta_6 LOSS2_{it} + \delta_7 LOSS3_{it} + \delta_8 LOSS4_{it} + \delta_9 DECREASE1_{it} + \delta_{10} DECREASE2_{it} + \delta_{11} DECREASE3_{it} + \delta_{12} DECREASE4_{it} + \delta_{13} \Delta ROA_{it} + \delta_{14} RET_{it} + \delta_{15} NEGFE1_{it} \times \Delta ROA_{it} + \delta_{16} NEGFE2_{it} \times \Delta ROA_{it} + \delta_{17} NEGFE3_{it} \times \Delta ROA_{it} + \delta_{18} NEGFE4_{it} \times \Delta ROA_{it} + \delta_{19} LOSS1_{it} \times \Delta ROA_{it} + \delta_{20} LOSS2_{it} \times \Delta ROA_{it} + \delta_{21} LOSS3 \times \Delta ROA_{it} + \delta_{22} LOSS4_{it} \times \Delta ROA_{it} + \delta_{23} DECREASE1_{it} \times \Delta ROA_{it} + \delta_{24} DECREASE2_{it} \times \Delta ROA_{it} + \delta_{25} DECREASE3_{it} \times \Delta ROA_{it} + \delta_{26} DECREASE4_{it} \times \Delta ROA_{it} + e_4$$

Estimated Coefficient (White Adjusted t-statistic)

	Benchmark			
Variable Name[a]	*Analyst Forecast*	*Prior Year*	*Zero or Positive*	*All Benchmarks*
Intercept	0.148	0.143	0.075	0.171
	(9.53)	(11.17)	(8.07)	(10.75)
NEGFE1	−0.035			−0.019
	(−1.72)			(−0.98)
NEGFE2	−0.126			−0.087
	(−5.55)			(−3.76)
NEGFE3	−0.211			−0.143
	(−8.41)			(−5.37)
NEGFE4	−0.197			−0.108
	(−7.06)			(−3.70)
DECREASE1		−0.056		−0.028
		(−2.62)		(−1.31)
DECREASE2		−0.156		−0.103
		(−6.96)		(−4.34)
DECREASE3		−0.208		−0.129
		(−7.36)		(−4.17)
DECREASE4		−0.295		−0.214
		(−7.99)		(−5.75)
LOSS1			−0.060	−0.003
			(−1.96)	(−0.10)
LOSS2			−0.021	0.051
			(−0.58)	(1.36)
LOSS3			−0.037	0.055
			(−0.53)	(0.77)

(*Continued on next page*)

benchmark for more than one quarter of the year. For example, the estimated coefficient (White's heteroscedasticity-consistent t-statistic) decreases from −0.035 (−1.72) when the firm misses the analyst forecast in one quarter, to −0.126 (−5.55) when the firm misses the analyst forecast in two quarters.

To evaluate the statistical significance of differences in the estimated coefficients, Panel A of Table 3 reports the results of Wald tests of these differences. The estimated coefficient

TABLE 3
Differences between the Estimated Coefficients Based on the Number of Quarterly Earnings Benchmarks Missed
(Wald tests for differences in the coefficient estimates from Table 2)

Panel A: Differences in Coefficient Estimates from Individual Benchmark Regressions
Difference in Coefficients [p − value]

Analyst Forecast Benchmark		
NEGFE2 − NEGFE1	*NEGFE3 − NEGFE2*	*NEGFE4 − NEGFE3*
−0.091 [0.000]	−0.085 [0.001]	0.014 [0.648]
Earnings of Same Quarter of Prior Year Benchmark		
DECREASE2 − DECREASE1	*DECREASE3 − DECREASE2*	*DECREASE4 − DECREASE3*
−0.100 [0.000]	−0.052 [0.097]	−0.087 [0.045]
Avoiding Loss Benchmark		
LOSS2 − LOSS1	*LOSS3 − LOSS2*	*LOSS4 − LOSS3*
0.039 [0.401]	−0.016 [0.843]	−0.008 [0.915]

Panel B: Differences in Coefficient Estimates from the Combined Regression
Difference in Coefficients [p–value]

Analyst Forecast Benchmark		
NEGFE2 − NEGFE1	*NEGFE3 − NEGFE2*	*NEGFE4 − NEGFE3*
−0.068 [0.003]	−0.056 [0.035]	0.035 [0.258]
Earnings of Same Quarter of Prior Year Benchmark		
DECREASE2 − DECREASE1	*DECREASE3 − DECREASE2*	*DECREASE4 − DECREASE3*
−0.075 [0.004]	−0.026 [0.422]	−0.085 [0.046]
Avoiding Loss Benchmark		
LOSS2 − LOSS1	*LOSS3 − LOSS2*	*LOSS4 − LOSS3*
0.054 [0.242]	0.004 [0.960]	−0.009 [0.912]

for missing the forecast benchmark in two quarters, NEGFE2, is significantly lower than the coefficient for missing the forecast in only one quarter, NEGFE1. Similarly, the estimated coefficient for missing the forecast benchmark in three quarters, NEGFE3, is significantly lower than the coefficient for NEGFE2. The estimated coefficient for NEGFE3, however, is not significantly different from the estimated coefficient for NEGFE4.

The results based on earnings-for-the-same-quarter-of-the-prior-year benchmark also support the hypothesis. Table 2 shows that the estimated coefficient for each of the four dummy variables is significantly negative. Similar to the analyst forecast benchmark, the estimated coefficient (t-statistic) decreases from −0.056 (−2.62) when the firm misses the benchmark (i.e., earnings decline) in one quarter, to −0.156 (−6.96) when the firm misses the benchmark in two quarters. The Wald tests in Panel A of Table 3 indicate that this decline is significant ($p < 0.01$). The estimated coefficients continue to decline as the firm records declining earnings in more quarters. However, the Wald tests are less significant as the number of quarters with declining earnings increases to three quarters and then to four quarters (p-value < 0.10 for DECREASE3 − DECREASE2 and p-value < 0.05 for DECREASE4 − DECREASE3).

On the other hand, the regression results of reporting a quarterly loss are less clear. Although Table 2 shows that the estimated coefficient for LOSS1 is significantly negative (as predicted), none of the other quarterly loss dummy variables are significant. The Wald tests in Panel A of Table 3 indicate that the estimated coefficients for the quarterly loss dummy variables are not significantly different from each other.

The fourth column of Table 2 presents the results when we include dummy variables for all of the benchmarks in a single regression. The results indicate that the change in the CEO's bonus is significantly lower if the firm misses the quarterly consensus analyst forecast more than once during the year. In addition, the CEO's bonus is significantly lower when the firm reports quarterly earnings below those of the same-quarter-of-the-prior-year more than once during the year.

Panel B of Table 3 presents the results from the Wald tests for differences in the coefficients estimated in the fourth column of Table 2. They indicate that the estimated coefficient for missing the forecast for two quarters (NEGFE2) is significantly more negative than the estimated coefficient for missing the forecast for one quarter (NEGFE1). Similarly, the estimated coefficient for missing the forecast for three quarters (NEGFE3) is significantly more negative than the estimated coefficient for NEGFE2 (p-values < 0.05). In addition, the estimated coefficient for reporting earnings below the earnings for the same quarter of the prior year for two quarters (DECREASE2) is significantly more negative than the estimated coefficient for missing the same benchmark for one quarter (DECREASE1). Finally, the estimated coefficient for reporting earnings below the earnings for the same quarter of the prior year for four quarters (DECREASE4) is significantly more negative (p-values < 0.05) than the estimated coefficient for missing the same benchmark for three quarters (DECREASE3).

Overall, the results support the contention that CEO bonuses are lower when the firm reports quarterly earnings below the consensus analyst forecast or below the earnings for the same quarter of the prior year. The effects are more pronounced when the firm misses the benchmark more than once during the year. Somewhat surprisingly however, we do not find that reporting losses adversely affects the CEO's bonus. Given our sample firms' overall high performance, avoiding reporting a loss could be a very low benchmark for these firms.

With regard to the control variables, the results confirm prior evidence that pay is positively associated with performance. The estimated coefficients for annual market (RET) and accounting (ΔROA) measures of performance are significantly positive in all four regressions.

The results are less clear for the interaction terms capturing the effect of missing quarterly earnings benchmarks on the pay-for-performance relation. Column 1 of Table 2 shows that for the analyst forecast benchmark, the estimated coefficients for the interactions of NEGFE3 and NEGFE4 with the change in annual ROA are significantly negative. However, they are not significant when all benchmarks are included in the regression (column 4 of Table 2).

Similarly, the estimated coefficients for the interaction of the earnings decline dummy variables with the change in annual ROA are generally not significant. The one exception is the unexpectedly positive coefficient for firms that report an earnings decrease in one-quarter, and only one-quarter, during the year. This result suggests that for such firms, the CEO's annual bonus compensation is more sensitive to annual accounting returns.

On the other hand, the estimated coefficients for the interaction of the quarterly loss dummy variables with change in annual ROA are all significantly negative. This is consistent with the literature that documents a weaker pay-for-performance relation for firms that report quarterly losses (Gaver and Gaver 1998), and with studies that show that CEO compensation is not affected by infrequent charges against earnings, such as restructuring charges (Dechow et al. 1994).

Overall, the results suggest that the effect of reporting quarterly losses (missing the zero earnings benchmark) on the CEO bonus differs from the effect of missing the other two quarterly benchmarks. Specifically, although quarterly losses appear to attenuate pay-performance sensitivity, there is no evidence that quarterly losses are associated with an incremental penalty on the CEO's bonus. In contrast, the evidence indicates that reporting earnings below the consensus analyst forecast or same-quarter-of-the-prior-year earnings benchmarks leads to a CEO bonus penalty and less attenuation of the pay-performance relation.

Estimating the Marginal Effect of Missing a Quarterly Earnings Benchmark

We use the coefficients reported in Table 2 to estimate the marginal effect on the CEO's bonus of missing a quarterly earnings benchmark. For each of the benchmark dummy variables, we evaluate the partial derivative of the regression equation at the median full sample ΔROA. We present the results for the individual benchmark regressions and for the combined regression in Panels A and B of Table 4.

For simplicity, we focus on the combined regression results in Panel B of Table 4. The results indicate that the penalty for missing any of the benchmarks for only one quarter during the year is relatively small (less than 3 percent of salary). However, if a firm reports earnings below the consensus analyst forecast for two quarters during the year, the average penalty approaches 9 percent of salary, and if the firm reports earnings below the same quarter of the previous year twice during the year, the average penalty exceeds 10 percent of the CEO's salary. For the analyst forecast benchmark, the maximum impact (14 percent of salary) occurs when the firm misses the benchmark for three quarters. For the prior-year earnings benchmark, the maximum impact (21 percent of salary) occurs when the firm reports earnings below the same quarter of the prior year for all four quarters.

On average, avoiding a quarterly loss appears to have a relatively small impact on the CEO's bonus. The penalty is just 0.4 percent of salary for reporting a loss in one quarter. Somewhat surprisingly, column 2 of Panel B in Table 4 indicates that, on average, CEOs receive higher bonuses if their firms report more than one quarterly loss. While this would

TABLE 4
Estimated Effect of Missing an Earnings Benchmark on Annual CEO Bonus Predicted Value of the Change in CEO Bonus as a Percentage of Salary[a]

Panel A: Marginal Impact Based on Individual Benchmark Regressions

	Benchmark		
Number of Quarters Missed	*Analyst Forecast*	*Prior Year*	*Avoiding a Loss*
1	−0.035	−0.054	−0.065
2	−0.126	−0.155	−0.023
3	−0.212	−0.208	−0.039
4	−0.199	−0.295	−0.049

Panel B: Marginal Impact Based on the Combined Regression

	Benchmark		
Number of Quarters Missed	*Analyst Forecast*	*Prior Year*	*Avoiding a Loss*
1	−0.020	−0.026	−0.004
2	−0.088	−0.103	0.050
3	−0.143	−0.129	0.053
4	−0.108	−0.214	0.043

[a] The predicted change in the CEO bonus as a percentage of prior-year salary, computed using the estimated coefficients from Table 2, evaluated at the sample median ΔROA.

be consistent with a "big bath" effect, where boards reward CEOs for "cleaning house" prior to turning companies around, this inference is weak because it is based on the insignificant positive intercept effects in Table 2.

Sensitivity Tests

We now examine the sensitivity of our results to alternative design specifications. We separate these tests into two categories: alternative measures of compensation and alternative functional forms. We briefly describe the rationale for each test, and the effects of the tests on our inferences. Complete results for these tests are available from the authors.

Alternative Compensation Measures

- Add stock options and restricted stock grants to the CEO's bonus

  Adding the value of stock option and restricted stock grants to the CEO's bonus provides a broader measure of compensation. However, the qualitative results from the individual benchmark tests are unchanged. On the other hand, when we include all of the benchmarks in a single regression, only DECREASE2 and DECREASE3 have significantly negative coefficients. Including equity grants in CEO compensation also reduces the explanatory power of performance measures, in general, and the significance of the benchmark dummy variables in particular. The adjusted R^2s for the regressions are approximately 2 percent, as opposed to the 16-18 percent when we use the CEO bonus as the compensation measure. Finding that the inferences generally hold, but are somewhat weaker when we add the stock option and restricted stock grants, is consistent with Core and Guay's (1999) contention that compensation committees grant equity securities to adjust the CEO's incentives, rather than as a reward for past performance. Our results are also consistent with past studies indicating that the pay-for-performance relation is generally stronger for cash compensation than for equity compensation (e.g., Baber et al. 1998; Gaver and Gaver 1998).

- Measure CEO compensation as the change in the log of salary plus bonus

  We address any potential biases resulting from the use of salary as a deflator by taking the change in the log of total cash compensation. Because some CEOs do not receive a bonus every year, we add salary to the bonus before taking the log. The study's inferences are unchanged.

Alternative Functional Specifications

- Estimate in ranks

  We estimate the regressions using ranks rather than cardinal values for the continuous variables to mitigate effects of extreme values. The results are qualitatively unchanged.

- Annual forecast error

  DeFond et al. (2001) document a positive relation between CEO bonuses and annual analyst forecast errors, after controlling for traditional accounting and market performance measures. Therefore, we add annual forecast error, defined as reported earnings per share less the consensus forecast at least 9 months before the end of the fiscal year, scaled by the absolute value of the forecast, as an explanatory variable. Including this variable in our regressions as an additional control for unexpected performance does not affect our inferences.

- Dummy variable for fourth quarter

  To ascertain whether the results are primarily attributable to the fourth quarter, we add a dummy variable equal to 1 if the firm missed its earnings forecast in the fourth quarter. We find that the estimated coefficient for the fourth-quarter dummy variable is not significantly different from zero, whereas the estimated coefficients for the quarterly benchmarks remain negative and significant. We conclude that missing the earnings forecast in the fourth quarter is not significantly different from missing the earnings forecast in the other quarters.

- Positive earnings subsample

  Prior research (e.g., Gaver and Gaver 1998) presents evidence that the pay-for-performance relation is asymmetric, depending on whether the firm reports positive or negative earnings. We therefore estimate our regressions on the subsample of firms that report positive annual earnings. The results are qualitatively unchanged.

- Partitions on size and performance

  Because our sample selection criteria yield a sample of large, successful firms we partition our sample based on the median of total assets (or the median annual stock return) and estimate the regressions separately for each size (return) subsample. Finding that the inferences hold for each subsample, including the smaller and less successful firms, suggests that our inferences are not likely an artifact of our sample selection criteria that led to a sample of relatively large and successful firms.

V. SUMMARY AND CONCLUSIONS

In this paper we examine whether the failure to meet quarterly earnings benchmarks is associated with a lower annual bonus for the CEO. We define three alternative earnings benchmarks: the consensus First Call analyst forecast; the earnings for the same quarter of the previous year; and zero. After controlling for the general pay-for-performance relation, we find a significant negative incremental effect on the CEO's bonus when the firm reports quarterly earnings below the analyst forecast, or below the earnings for the same quarter of the prior year, for at least two quarters during the year. The effect on the CEO's bonus of reporting quarterly losses is considerably weaker and not consistently significant. Overall, our findings are consistent with CEO bonus payments' providing CEOs with economic incentives to meet the consensus analyst forecast and to report earnings at least equal to the same quarter of the prior year.

One limitation of our study is that we do not observe executive compensation contracts that impose explicit penalties on managers for failing to meet quarterly earnings benchmarks. As a result, we cannot document a direct link between the failure to meet a benchmark and a compensation penalty. Murphy (1999) examines actual compensation contracts and notes that prior-year earnings are, at times, explicitly part of compensation contracts. However, compensation committees do not appear to explicitly use analyst forecasts or loss avoidance as benchmarks in CEO bonus plans. Instead, these benchmarks likely affect the compensation committee's use of discretion in determining the magnitude and allocation of the bonus pool.

Our results contribute to the literature by documenting that CEOs have a bonus incentive to manage earnings to meet benchmarks. Our evidence has implications for studies of earnings management around various quarterly benchmarks. While the bonus penalty for missing the benchmark for just one quarter during the year is relatively small, the penalty increases substantially if the firm misses the benchmark in more than one quarter during the year. This expectation of a lower bonus may provide managers with an incentive to make more aggressive accounting choices. Our evidence also provides additional insight into the pay-for-performance relation. Prior studies document weaker pay-for-performance relations for firms reporting losses than for firms reporting profits, suggesting that CEO compensation is at least partially protected against unfavorable outcomes (see Dechow et al. 1994; Gaver and Gaver 1998). Although we find similar results for the interaction between missed benchmarks and changes in ROA, our evidence nonetheless suggests that CEO compensation is not fully protected, as CEOs are *incrementally* penalized for failing to meet analyst forecast and prior year earnings benchmarks.

The finding that managers appear to have a bonus incentive to meet quarterly earnings benchmarks also has implications for the SEC's recent concerns that managers may compromise faithful representation to satisfy Wall Street (Levitt 1998). Our results suggest that incentives to manage earnings stem from internal sources such as the board of directors, as well as external sources such as the capital market. Thus, the attainment of the earnings benchmarks appears to be a desirable performance measure and earnings management is a cost of using that measure. As a result, although the SEC could reduce opportunistic reporting by encouraging boards to actively monitor corporate financial reporting, this benefit must be weighed against the additional monitoring cost.

While our results are consistent with CEO bonuses driving the earnings patterns observed around analyst forecasts and the earnings from the same quarter of the prior year, our evidence does not explain the unusual earnings patterns around the zero earnings benchmark documented in prior earnings management studies (Burgstahler 1997; Burgstahler and Dichev 1997). One reason might be that our sample consists of larger and more successful firms that report a quarterly loss in only 6 percent of the total firm-quarters. While the benchmarks used to evaluate these firms' management could differ from those of a broader population, our inferences hold in our small firm and low stock return subsamples. Thus, sample composition is unlikely to explain the difference between our finding of little or no bonus incentive to avoid a loss and earnings management studies' conclusion that avoiding a loss appears to be the most important benchmark. Perhaps factors other than bonuses (such as job security concerns) motivate CEOs to avoid losses. We leave the relation between achieving earnings benchmarks and CEO turnover to future research.

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Strategic Interaction in Auditing: An Analysis of Auditors' Legal Liability, Internal Control System Quality, and Audit Effort

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ABSTRACT: This paper presents a model in which a firm's owner, an auditor, and outside investors strategically interact. The owner's investment in the quality of the firm's internal control system and the auditor's effort jointly affect the informativeness of the auditor's report on the firm's financial statements. If the auditor's legal liability to investors is large, then an efficiency loss arises because the owner underinvests in the internal control system and the auditor overinvests effort. On the other hand, if the liability is small, then an efficiency loss arises from the owner's overinvestment and the auditor's underinvestment. Regulators can improve allocative efficiency by changing the auditor's legal liability. However, in our model, it is impossible to completely eliminate the efficiency loss by changing the auditor's liability alone, because no damage award can induce both the owner and auditor to make socially optimal investments in the internal control system and audit effort. We also interpret recent changes in the regulatory environment in the context of our model.

Keywords: *auditors' legal liability; internal control system quality; audit effort.*

I. INTRODUCTION

This paper studies the economic consequences of auditors' legal liability in a setting where the quality of a firm's internal control system and an auditor's effort jointly determine the informativeness of the auditor's report on the firm's financial

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statements.[1] Recent studies in the auditor legal liability literature—such as Dye (1993, 1995), Narayanan (1994), Schwartz (1997), Chan and Pae (1998), Hillegeist (1999), Zhang and Thoman (1999)—have focused on audit effort, but have not considered the link between audit effort and the firm's internal control system. Our study considers this link. This feature of the paper provides new insight into the efficiency implications of auditors' legal liability.

The costly nature of the audit effort and internal control system raises a question of how to balance the owner's investment in the internal control system with the auditor's effort. We show that when the auditor is legally liable to financial statement users, there is a nontrivial strategic interaction between the auditor's provision of effort and the firm owner's investment in the internal control system. If the owner and auditor fully cooperate, then they could achieve an efficient resource allocation. However, in a noncooperative setting in which the owner and auditor maximize their own personal payoffs, an efficiency loss arises from a misallocation of resources. We first examine how a change in the auditor's legal liability to outside investors (who trade based on audit reports) affects the firm owner's choice of internal control system quality and the auditor's effort choice. We then investigate how changes in these equilibrium choices (induced by a change in the auditor's legal liability) influence social welfare, which we define as the sum of the owner's, auditor's, and outside investors' *ex ante* expected payoffs.

Our model is similar to Schwartz (1997), but we extend her model as follows. At the beginning of the period, a firm's owner makes an investment that determines the quality of her firm's internal control system. The owner then hires an auditor, who (after assessing the quality of the internal control system) provides unobservable effort and issues an audit report. The audit report is the auditor's public attestation to the firm's future prospects, which we refer to as the firm's type.[2] In contrast to prior research, we allow not only the auditor's effort, but also the quality of the firm's internal control system to affect the informativeness of the audit report about the firm's type. In particular, we assume that: (1) the higher the quality of the internal control system, the higher the probability that the auditor correctly attests to the firm's type; and (2) the higher the quality of the internal control system, the lower the marginal impact of audit effort on the probability of the correct audit report. The first assumption states that, for a given level of audit effort, an increase in the firm's internal control system quality improves the audit report's informativeness about the firm's type; whereas the second assumption states that an increase in audit effort has a greater incremental effect on the informativeness of the audit report when the quality of the internal control system is low (relative to when it is high). The second assumption implies that audit effort and internal control system quality are strategic substitutes.[3] Investors in a competitive capital market price the firm based on an audit report, and if they purchase this firm, they make an additional capital investment. At the end of the period, the firm's cash flow is realized, and the firm's true type is publicly revealed if the firm goes bankrupt. The investors sue the auditor if the firm's true type is different from the type attested to by the auditor (i.e., if an audit failure has occurred). In that case, the auditor must pay damages to investors.

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[1] We broadly define an internal control system to include hiring appropriate personnel, installing a reliable treasury/accounting system, having competent audit committee members, etc. Although a high-quality internal control system has many benefits, our main focus in this paper is on its spillover effect on external auditing.

[2] Auditors, in reality, do not make a direct statement about their clients' future prospects; instead, they express an opinion about whether a firm's financial statements are presented in accordance with Generally Accepted Accounting Principles (GAAP). However, outsiders (e.g., investors, creditors, financial analysts, investment banks, etc.) then use the audited financial statements to predict firms' future prospects. For example, when investors assess a firm's future earnings and/or its viability as a going concern, they rely on the information in financial statements. When a firm applies for a loan, the bank requires the firm to submit financial statements and uses that information to make a loan decision. In this paper, for the sake of expositional simplicity, we refer to the auditor as directly reporting on the firm's future prospects.

The paper's main results are as follows. First, we show that an increase in the auditor's legal liability reduces the owner's investment in the internal control system and motivates the auditor to provide more effort. The intuition is straightforward. The auditor, faced with increased legal liability, has a stronger *ex ante* incentive to avoid an audit failure by exerting more effort. On the other hand, as the auditor's legal liability increases, outside investors pay more for the firm because of the audit's increased insurance value. This reduces the owner's *ex ante* incentive to choose a high-quality internal control system.

Second, for all levels of the auditor's legal liability, the equilibrium allocation (i.e., the owner's investment in the internal control system and the auditor's effort provision) differs from the first-best allocation that maximizes social welfare. As a result, there always exists an efficiency loss. The way in which resources are misallocated depends on the size of the auditor's legal liability. For example, when the auditor's legal liability is sufficiently large, the owner has little incentive to choose a high-quality internal control system, but the auditor has a strong incentive to avoid an audit failure by providing more effort. In equilibrium, the owner underinvests while the auditor overinvests, relative to their first-best investments. On the other hand, when the auditor's liability is sufficiently small, incentives are reversed so that the efficiency loss arises from the owner's overinvestment and the auditor's underinvestment. Indeed, we show that there does *not* exist a level of auditor legal liability that induces *both* the owner *and* auditor to choose the first-best investments. In particular, a liability level that induces one party's first-best investment always results in the other party's underinvestment.

Our result that an efficiency loss is inevitable contrasts with Schwartz's (1997) main result. Specifically, she concludes that a regulator *can* achieve a socially efficient resource allocation (i.e., the first-best outcome in her model) by choosing an appropriate damage award to investors. Our extension of Schwartz's (1997) model to include a firm owner's strategic choice of the internal control system quality that affects the informativeness of the audit report, leads to a very different conclusion: there is *no* damage award that induces both the socially optimal internal control system quality and the socially optimal audit effort. In other words, regulators *cannot* achieve the first-best outcome by choosing the damage award *alone*.[4]

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[3] See Bulow et al. (1985) for detailed explanations of strategic substitutability in a gaming situation. The key implication of this assumption in our model is that, *ceteris paribus*, when the client firm has a higher-quality internal control system, it is rational for the auditor to reduce his effort (measured by the auditor's costs of conducting an audit). Such an inverse relation between the quality of the internal control system and audit effort is consistent with practice (e.g., see Wallace 1984).

[4] To be more precise, the regulator in Schwartz's (1997) model has two problems. One is to control investors' incentive to overinvest, which arises when the amount of their potential *ex post* damage recovery from the auditor depends on their *ex ante* investment. This incentive problem disappears if the regulator makes the damage award *independent* of the investors' actual investment. The regulator's remaining problem is then how to motivate the auditor to provide the socially efficient level of audit effort. Schwartz (1997) shows that the regulator can do so by choosing an appropriate damage award (independent of the actual investment), and this achieves the first-best

(*Continued on next page*)

Our third main result illustrates how regulation of the auditor's legal liability affects the equilibrium social welfare. We show that if the auditor's legal liability is sufficiently large, then reducing the liability improves social welfare by alleviating the owner's extreme underinvestment and the auditor's overinvestment.[5] On the contrary, if the auditor's liability is sufficiently small, increasing liability improves social welfare because it alleviates the auditor's extreme underprovision of effort and the owner's overinvestment. Indeed, we show that there exists a level of auditor liability that minimizes (but does not eliminate) the efficiency loss.

The rest of this paper is organized as follows. Section II presents the model. In Section III, we characterize the socially optimal allocation of resources in our model. In Section IV, we derive the equilibrium allocation. We present our main results in Section V and examine the economic consequences of a change in auditors' legal liability and discuss some regulatory implications of our analysis. Section VI summarizes the paper.

II. THE MODEL

All parties in the model have risk-neutral preferences and a zero discount rate for their future payoffs. Consider a setting in which a firm's owner wants to sell the firm to outside investors in a competitive capital market. The firm has a risky project, which requires a fixed amount of capital, $I > 0$. If outside investors purchase this firm, they supply I and undertake the project. The gross return from the project, which is the firm's only cash flow, is $R > I$ if the project succeeds, but zero if it fails. The probability that the project will succeed depends on its type, which can be either high (H) or low (L). *A priori*, all parties in the model have common prior beliefs that the project type is high with probability $\lambda \in (0, 1)$.

Let $p_t \in (0, 1)$, $t = H, L$, be the probability that a type-t project will succeed, and assume that a high-type project is more likely to succeed (i.e., $p_L < p_H$). Further assume that:

$$p_L R < I < p_H R \qquad \text{and} \qquad NPV_\lambda \equiv [\lambda p_H + (1 - \lambda) p_L]R - I > 0. \tag{1}$$

That is, a high- (low-) type project has a positive (negative) net present value while, based on prior beliefs, the project has a positive net present value on average. Given that the capital market is competitive, the owner sells the firm at the price of NPV_λ, which is the owner's payoff. However, the lack of information about the project type gives rise to an *ex ante* efficiency loss. Specifically, if the project type were common knowledge, investors would undertake only a high-type project, in which case the firm value would equal $p_H R - I$. This implies that the owner's *ex ante* welfare loss amounts to $\lambda(p_H R - I) - NPV_\lambda = (1 - \lambda)(I - p_L R) > 0$, and thus, she has an incentive to obtain more precise information about the project type than prior beliefs.

Footnote 4 (Continued)
outcome in her model. In contrast to Schwartz's (1997) model, the informativeness of the audit report in our model is a *joint* product of the owner's investment in the internal control system and the auditor's effort investment. Consequently, the regulator cannot *simultaneously* control the owner and auditor's incentives by using a *single* regulatory instrument (i.e., the damage award).

[5] Shibano (1996) and Chan and Pae (1998) establish a similar result that a decrease in the auditor's legal liability may increase social efficiency. However, they solely focus on the efficiency implication of a change in audit effort (implied by a change in the legal liability) without considering how a firm's endogenous choice of the internal control system affects social efficiency. In contrast, our analysis shows that the efficiency gain comes not only from attenuating the auditor's overprovision of effort, but also from an improvement in the internal control system.

Financial statements provide valuable information about a firm's future prospects because they summarize the firm's current economic status, which is a leading indicator of its future profitability (such as the project type in our model). Following the literature (e.g., Dye 1993, 1995; Narayanan 1994; Schwartz 1997; Chan and Pae 1998; Hillegeist 1999; Lee et al. 1999; Zhang and Thoman 1999), we assume that an auditor issues an attestation report on the project type. However, in contrast to this prior research, we postulate that the audit report's informativeness about the project type hinges on *both* the auditor's effort *and* the quality of the firm's internal control system (hereafter ICS) because the auditor's assessment depends not only on his own expertise, but also on the quality of the firm's financial statements. *Ceteris paribus*, financial statements' quality (i.e., how well they represent a firm's economic reality) depends on the quality of the firm's ICS.[6]

Formally, the sequence of events is as follows. At the beginning of the period, the owner makes an investment, $q_o \geq 0$, which represents the quality of the firm's ICS. The owner then hires an auditor from a competitive audit market at a noncontingent audit fee, $K \geq 0$, and the audit process begins.[7] The auditor's costly effort throughout the audit process is denoted by $q_a \geq 0$. We assume that q_a is unobservable to the other parties in the model. There are two stages in the audit process. At the first stage, the auditor evaluates the firm's ICS, after which he gains knowledge of q_o. At the second stage, the auditor collects and verifies evidence of the firm's economic reality. The auditor's decision at this second stage depends on his knowledge of the firm's ICS quality obtained at the first stage. At the end of the audit process, the auditor issues a report on q_o and the firm's future prospects, i.e., the project type. We assume that the auditor's report on q_o is without error. On the other hand, his report on the project type may be erroneous. Specifically, given the project's true type, we assume that the pair of ICS quality and audit effort, $\mathbf{q} \equiv (q_o, q_a) \in \Re_+^2$, translates into the conditional probabilities of the auditor's correct attestation to the project type as follows:

$$\Pr[h \mid H, \mathbf{q}] = 1 \quad \text{and} \quad \Pr[\ell \mid L, \mathbf{q}] = \phi(\mathbf{q}), \tag{2}$$

where h (ℓ) is the auditor's attestation report that the project type is high (low). The audit technology given above captures the unique feature of our model in that both the audit effort, q_a, and ICS quality, q_o, affect the probability distribution of audit reports.[8]

We impose the following regularity conditions on audit technology ϕ. For all $\mathbf{q} \in \Re_+^2$:

(i) $\phi(\mathbf{q}) \in [0, 1]$;
(ii) $\phi_i(\mathbf{q}) > 0$ and $\phi_{ii}(\mathbf{q}) < 0$ for all $i \in \{o, a\}$ and $\phi_{oo}(\mathbf{q})\phi_{aa}(\mathbf{q}) - [\phi_{oa}(\mathbf{q})]^2 > 0$,

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[6] A presumption here is that, *ceteris paribus*, when a firm has a high-quality ICS, its financial statements are less likely to contain (intentional and/or unintentional) misreporting and, thus, they are more useful in predicting the firm's future prospects. Although our model exclusively focuses on how the owner's choice of an ICS quality affects the auditor's effort choice in an external reporting context, a high-quality ICS can lead to other benefits. For example, a high-quality ICS can reduce agency costs within the firm. An earlier version of this paper considered a setting in which the firm's owner hires a manager and the manager's unobservable effort improves the project's future cash flow in the sense of first-order stochastic dominance. In that setting, we showed that when the owner has a higher-quality ICS, she induces the manager's costly effort more efficiently (i.e., with a lower expected wage).

[7] For analytic simplicity, we treat K as an exogenous variable. However, K could be endogenous without affecting the paper's results. This follows because, as long as the audit fee is not contingent on any subsequent events and observables (including audit reports, the firm's ICS quality and its selling price, the return from the project, etc.), it does not affect the owner and auditor's decisions.

[8] Although the audit technology assumed here implies no type-I error, a weaker assumption would not change the paper's results. As Dye (1995, 80) noted, one could introduce a type-I error (i.e., $\Pr[\ell \mid H, \mathbf{q}] > 0$), and if this type-I error rate is sufficiently small that the project is implemented only when audit report h is issued, then the paper's analysis follows through.

where:

$$\phi_i(\mathbf{q}) \equiv \frac{\partial\phi(\mathbf{q})}{\partial q_i} \quad \text{and} \quad \phi_{ij}(\mathbf{q}) \equiv \frac{\partial^2\phi(\mathbf{q})}{\partial q_i \partial q_j} \quad \text{for } i, j \in \{o, a\};$$

(iii) $\lim_{q_i \to 0} \phi_i(\mathbf{q}) = \infty$ and $\lim_{q_i \to \infty} \phi_i(\mathbf{q}) = 0$ for all $i \in \{o, a\}$;

(iv) $\phi_{oa}(\mathbf{q}) = \phi_{ao}(\mathbf{q}) < 0$.

The first condition states that ϕ is a probability function. The second condition states that ϕ is an increasing and concave function of $\mathbf{q}$. Hence, *ceteris paribus*, an increase in ICS quality enhances ϕ (at a decreasing rate), and so does the audit effort. In this sense, q_o and q_a are complements in improving the audit report's informativeness about the project type. The third condition ensures a unique interior optimum for a social surplus maximization problem, which we will use as a benchmark in our analysis. The fourth condition states that an increase in q_o reduces q_a's *marginal* effect on ϕ, and vice versa. This implies that q_o and q_a are *strategic substitutes* (in the sense of Bulow et al. 1985).[9]

After observing the auditor's report on q_o and the project type (i.e., ℓ or h), outside investors price the firm accordingly. Investors' knowledge of q_o implies that they can correctly infer the auditor's unobservable effort q_a since, as will be shown, there is a one-to-one correspondence between q_o and q_a. When the auditor issues report ℓ, the owner cannot sell the firm at a positive price because $\Pr[L \mid \ell, \mathbf{q}] = 1$ for any $\mathbf{q}$ and the net present value of a low-type project is negative. Thus, investors do not purchase the firm and the game is over. On the other hand, if the auditor issues report h, equations in (2) imply that investors revise their beliefs about a high-type project to:

$$\mu(\mathbf{q}) \equiv \Pr[H \mid h, \mathbf{q}] = \frac{\lambda}{\lambda + (1 - \lambda)(1 - \phi(\mathbf{q}))}. \tag{3}$$

Since $\mu(\mathbf{q}) \geq \lambda$ for any $\mathbf{q}$, investors purchase the firm and supply capital I to undertake the project. The return from the project is realized at the end of the period and it is publicly observed. If the project succeeds (i.e., the return is R), the game is over. However, if the firm goes bankrupt (i.e., the return is zero), the project's true type becomes public information. As a result, investors know whether the auditor correctly attested to the project type (through the audit report). Given that the auditor issued report h, we say that an audit failure has occurred if the project's true type is L. We assume that the auditor is held liable for the audit failure (i.e., a strict liability rule prevails) and $D > 0$ is the auditor's expected liability payment to investors. The game structure is common knowledge. Appendix A lists the notation and Figure 1 is an extensive-form game tree of the model.

Before proceeding, we note that the present model directly applies to settings where the owner raises capital I from outsiders rather than selling the firm in its entirety. For example, one might consider a case in which the owner raises capital from investors in

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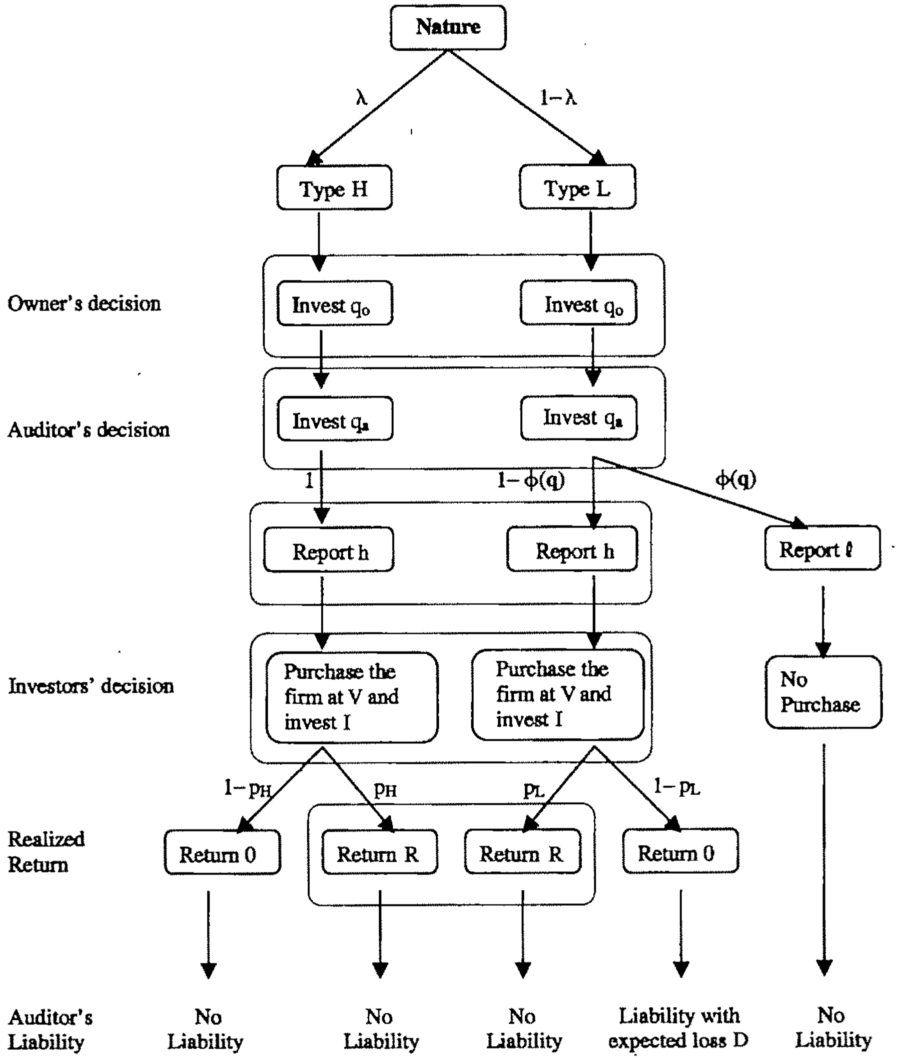
[9] As we show in Section IV, the key implication of $\phi_{ao} < 0$ is that when ICS quality is high, it is optimal for the auditor to reduce his costly effort. This assumption is plausible (e.g., Wallace 1984, 20; O'Keefe et al. 1994, 251–252). For example, consider two levels of ICS quality, q_o^1 and q_o^2 with $q_o^1 < q_o^2$. In an audit sampling to verify the account balance of a financial statement item, q_a is positively related to the sample size. The condition, $\phi_a(q_o^1, q_a) > \phi_a(q_o^2, q_a)$, then implies that increasing the sample size has a smaller incremental informational value (i.e., a smaller positive impact on ϕ) when ICS quality is high than when ICS quality is low. This is reasonable if a higher-quality ICS screens out intentional/unintentional errors so that the reported account balance is already closer to the true balance, and there is less scope for audit effort to improve the accuracy of the financial statements.

FIGURE 1
An Extensive-Form Game Tree Representation of the Owner's, Auditor's, and Investors' Decision Problems



return for the firm's equity, or another case in which the owner funds the project through a loan contract with a bank. None of this paper's results change in these cases. What matters in our analysis is that third parties make their capital supply decision based on the auditor's report, and they sue the auditor in the case of an audit failure.

III. SOCIAL SURPLUS AND BENCHMARK

Social Surplus

We define social surplus as the sum of the owner's, investors', and auditor's *ex ante* expected payoffs. Let $\mathbf{q} = (q_o, q_a)$ be given. Since investors will purchase the firm and undertake the project if and only if audit report h is issued, the competitive market value of the firm given audit report h is:

$$V(\mathbf{q}) = \{[\mu(\mathbf{q})p_H + (1 - \mu(\mathbf{q}))p_L]R - I\} + [1 - \mu(\mathbf{q})](1 - p_L)D, \qquad (4)$$

where $\mu(\mathbf{q})$ is given by equation (3). The expression in the braces is the investors' net expected return from the project, and the last term is their expected payoff from litigation against the auditor. It follows that, for any given K and $\mathbf{q}$, the owner's *ex ante* expected payoff is:

$$\begin{aligned}\Pi^O(\mathbf{q}) &= \Pr[h \mid \mathbf{q}]V(\mathbf{q}) - K - q_o \\ &= NPV_\lambda + (1 - \lambda)\phi(\mathbf{q})(I - p_LR) + (1 - \lambda)(1 - \phi(\mathbf{q}))(1 - p_L)D - K - q_o, \qquad (5)\end{aligned}$$

where $\Pr[h \mid \mathbf{q}] \equiv \lambda + (1 - \lambda)(1 - \phi(\mathbf{q}))$ is the probability that audit report h is issued, and we use equations (1), (3), and (4) in the second equality. On the other hand, given K and $\mathbf{q}$, the auditor's *ex ante* expected payoff is:

$$\Pi^A(\mathbf{q}) = K - q_a - (1 - \lambda)(1 - \phi(\mathbf{q}))(1 - p_L)D, \qquad (6)$$

where the last term is his *ex ante* expected liability loss.

Since investors' expected payoff is zero in the competitive capital market (which we have already taken into account in equation [4]), social surplus in our model is equal to:

$$\Pi^S(\mathbf{q}) \equiv \Pi^O(\mathbf{q}) + \Pi^A(\mathbf{q}) = NPV_\lambda + (1 - \lambda)\phi(\mathbf{q})(I - p_LR) - (q_o + q_a), \qquad (7)$$

where we use equations (5) and (6). It is clear that the audit fee, the auditor's liability loss, and the equilibrium value of the firm do not appear in equation (7) because they are mere wealth transfers; instead, social surplus is solely characterized by $\mathbf{q}$. The second term in equation (7) represents the *ex ante* social expected benefit from detecting a low-type project (i.e., savings on wasteful investment), and the last term is the social cost associated with that benefit.

Benchmark: The First-Best Case

Before proceeding to a formal analysis of the model, we consider a hypothetical case, referred to as the first-best case, in which a regulator (or a social planner) can directly enforce the owner and auditor to choose particular levels of q_o and q_a to maximize the social surplus given in equation (7). That is, consider a regulator's problem:

$$\max_{q_o, q_a} \Pi^S(\mathbf{q}) = NPV_\lambda + (1 - \lambda)\phi(\mathbf{q})(I - p_LR) - (q_o + q_a).$$

Given our assumptions on ϕ, a socially optimal pair of ICS quality and audit effort is uniquely characterized by the following first-order conditions:

$$(1 - \lambda)(I - p_LR)\phi_o(\mathbf{q}) = 1; \qquad (8)$$

$$(1 - \lambda)(I - p_LR)\phi_a(\mathbf{q}) = 1. \qquad (9)$$

Let $\mathbf{q}^\dagger \equiv (q_o^\dagger, q_a^\dagger)$ be the solution to equations (8) and (9), and we call it the first-best allocation. That is, $\mathbf{q}^\dagger$ is an allocation at which the social marginal cost and benefit of $\mathbf{q}$ are equal.

It is useful to view equations (8) and (9) as indicating how the owner and auditor should coordinate q_o and q_a to maximize social surplus. In particular, solve equation (8) for q_o as a function of q_a and denote it by $q_o^F(q_a)$. Similarly, solve equation (9) for q_a as a function of q_o and denote it by $q_a^F(q_o)$. We call $q_o^F(q_a)$ and $q_a^F(q_o)$ *coordination functions* in the first-best case. Given the concavity of ϕ and $\phi_{oa} = \phi_{ao} < 0$, totally differentiating the first-order conditions reveals that both $q_o^F(q_a)$ and $q_a^F(q_o)$ are decreasing functions. This means that when ICS quality is higher, the auditor invests less effort, and vice versa. In the space of $(q_o, q_a) \in \Re_+^2$, the first-best allocation $\mathbf{q}^\dagger$ is the point at which the two coordination functions, i.e., $q_o^F(q_a)$ and $q_a^F(q_o)$, intersect (see Figure 2).

IV. EQUILIBRIUM ALLOCATION OF ICS QUALITY AND AUDIT EFFORT

In the previous section, we characterized the socially optimal allocation of q_o and q_a in the first-best case, i.e., $\mathbf{q}^\dagger$. However, the first-best case is hypothetical because we have assumed that a regulator can directly enforce the owner and auditor to choose $\mathbf{q}^\dagger$ to maximize social surplus. We now return to our original model, referred to as the second-best case, in which the owner and auditor choose q_o and q_a based on their private incentives—i.e., to maximize their own surpluses. In this case, the owner's incentive for ICS quality and the auditor's incentive for audit effort are not aligned to maximize social surplus. Consequently, the equilibrium allocation of $\mathbf{q}$ is different from $\mathbf{q}^\dagger$, which implies that an efficiency loss exists in the second-best case. We formalize this argument in what follows.

We derive the equilibrium of the game by using backward induction. First, for any given $\mathbf{q}$, the firm value in the competitive capital market when the auditor issues report h equals $V(\mathbf{q})$ in equation (4). Next, consider the game between owner and auditor. The owner moves first by choosing an ICS quality, q_o. Then, after observing q_o, the auditor determines his audit effort, q_a. This implies that the owner and auditor play the roles of a Stackelberg leader and follower, respectively. To derive a Stackelberg equilibrium, consider the auditor's problem first.

Given an audit fee and his *ex ante* expected liability loss, as stated in equation (6), the auditor solves:

$$\max_{q_a} \Pi^A(\mathbf{q}) = K - q_a - (1 - \lambda)(1 - \phi(\mathbf{q}))(1 - p_L)D.$$

Let $q_a^r(q_o, D)$ be the auditor's best response to q_o given D. Since $D > 0$, it follows that $q_a^r(q_o, D)$ is the value of q_a that solves the first-order condition:

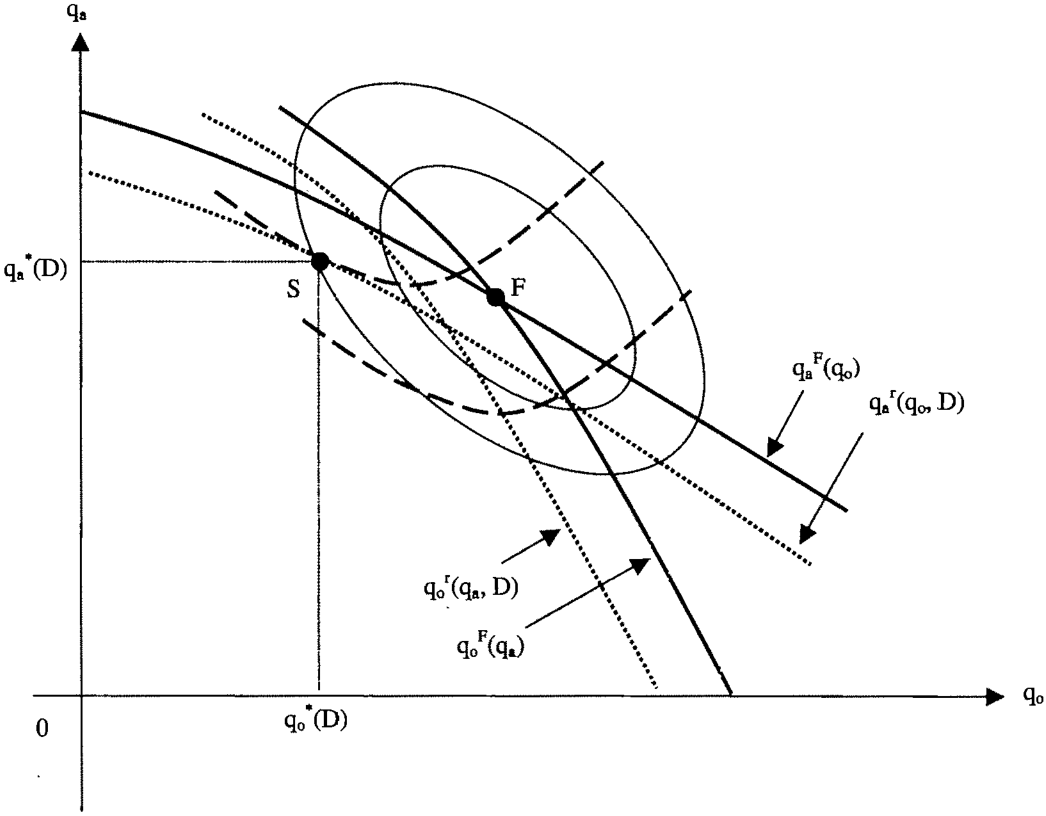
$$(1 - \lambda)(1 - p_L)D\phi_a(\mathbf{q}) = 1. \tag{10}$$

Given equation (10) and the concavity of ϕ, the auditor increases effort when ICS quality is lower (i.e., $\partial q_a^r/\partial q_o = -\phi_{ao}/\phi_{aa} < 0$) because q_o and q_a are strategic substitutes (i.e., $\phi_{ao} < 0$). Also, given equation (10) and the monotonicity of ϕ (i.e., $\phi_a > 0$), we see that the auditor increases effort when his expected liability is larger (i.e., $\partial q_a^r/\partial D = -\phi_a/D\phi_{aa} > 0$). In addition, a comparison of equations (9) and (10) reveals that:

$$q_a^r(q_o, D) = q_a^F(q_o) \text{ for any } q_o \text{ if } D = \overline{D} \equiv \frac{I - p_L R}{1 - p_L}. \tag{11}$$

In other words, when $D = \overline{D}$, the auditor's reaction function is the same as the coordination

FIGURE 2
The First-Best Allocation of Internal Control System Quality and Audit Effort, and the Second-Best Equilibrium



$q_o^F(q_a)$ and $q_a^F(q_o)$ are the coordination functions in the first-best case. The intersection of $q_o^F(q_a)$ and $q_a^F(q_o)$, denoted by F, is the first-best allocation, $\mathbf{q}^\dagger$. Tilted ellipses are iso-social-surplus curves, and the bigger ones (i.e., the ones farther from F) correspond to lower levels of social surplus.

$q_a^r(q_o, D)$ is the auditor's best response function, and $q_o^r(q_a, D)$ is the owner's best response function *if* the auditor does *not* observe q_o. U-shaped dashed parabolas are the owner's iso-surplus curves. The equilibrium allocation, $\mathbf{q}^*(D) \equiv (q_o^*(D), q_a^*(D))$, is denoted by S at which the owner's iso-surplus curve is tangential to $q_a^r(q_o, D)$.

function in the first-best case. This follows because $\overline{D}$ is the value of the auditor's expected liability that induces the auditor to fully internalize the economic consequence of investment in a low-type project.

Now consider the owner's problem. Given $D > 0$, the owner anticipates that the auditor will provide effort $q_a^r(q_o, D)$ if she chooses q_o. This implies that the auditor will issue report h with probability $\Pr[h \mid \mathbf{q}] = \lambda + (1 - \lambda)(1 - \phi(\mathbf{q}))$, in which case outside investors price the firm at $V(\mathbf{q})$ given in equation (4). Thus, using equation (5) along with the fact that the auditor's effort choice hinges upon ICS quality, we can state the owner's problem as:

$$\max_{q_o} \Pi^O(q_o, q_a^r(q_o, D), D) = NPV_\lambda + (1-\lambda)\phi(q_o, q_a^r(q_o, D))(I - p_L R)$$
$$+ (1-\lambda)[1 - \phi(q_o, q_a^r(q_o, D))](1 - p_L)D - K - q_o, \quad (12)$$

where $q_a^r(q_o, D)$ is given by equation (10). Note that if $D = \bar{D}$, the owner's objective function reduces to:

$$\Pi^O = NPV_\lambda + (1-\lambda)(I - p_L R) - K - q_o = \lambda(p_H R - I) - K - q_o, \quad (13)$$

which indicates that the owner is fully insured against the negative net present value investment in a low-type project.[10] If so, as evident in equation (13), the owner's optimal choice of ICS quality is zero. To focus on the interior solution to the owner's problem, we restrict our subsequent analysis to the case of $D < \bar{D}$.

Given $D \in (0, \bar{D})$, the owner's optimal choice of ICS quality is characterized by the first-order condition for the owner's maximization problem:

$$(1-\lambda)[(I - p_L R) - (1 - p_L)D]\left(\phi_o(q_o, q_a^r(q_o, D)) + \phi_a(q_o, q_a^r(q_o, D))\frac{\partial q_a^r(q_o, D)}{\partial q_o}\right) = 1. \quad (14)$$

Let $q_o^*(D)$ be the solution to equation (14), which is the owner's equilibrium choice of ICS quality. Substituting $q_o^*(D)$ into the auditor's best response function yields the equilibrium audit effort; i.e., it is given by $q_a^*(D) \equiv q_a^r(q_o^*(D), D)$, where $q_a^r(q_o, D)$ is characterized by equation (10). Let $\mathbf{q}^*(D) \equiv (q_o^*(D), q_a^*(D))$ be the equilibrium allocation given D.

Figure 2 depicts the equilibrium allocation $\mathbf{q}^*(D)$ and the first-best allocation $\mathbf{q}^\dagger$. First, we explain the first-best allocation. As explained in Section III, $q_o^F(q_a)$ and $q_a^F(q_o)$ are the coordination functions in the first-best case so that the first-best allocation $\mathbf{q}^\dagger$ is the intersection of $q_o^F(q_a)$ and $q_a^F(q_o)$, denoted by F. Tilted ellipses are iso-social-surplus curves, and the bigger ones (i.e., the ones farther from F) correspond to lower levels of social surplus.

Next, we explain the second-best case. In Figure 2, $q_a^r(q_o, D)$ is the auditor's best response function defined by equation (10). For an expositional purpose, let $q_o^r(q_a, D)$ be the value of q_o defined by equation (14) with $\partial q_a^r/\partial q_o = 0$. That is, $q_o^r(q_a, D)$ is the owner's best response function if the auditor does not observe q_o. In contrast to the coordination functions in the first-best case, both the owner's and the auditor's best response functions hinge upon D. In particular, for any given $D < \bar{D}$, we have $q_a^r(q_o, D) < q_a^F(q_o)$ for all q_o. This is because, for any given q_o, the auditor's private marginal benefit of audit effort (given by the left-hand side of equation [10]) is strictly less than the social marginal benefit of audit effort (given by the left-hand side of equation [9]). Similarly, we have $q_o^r(q_a, D) < q_o^F(q_a)$ for all q_a because the owner's private marginal benefit of q_o (given by the left-hand side of equation [14] with $\partial q_a^r/\partial q_o = 0$) is strictly less than the social marginal benefit of q_o (given by the left-hand side of equation [8]). Finding the owner's optimal ICS quality given the auditor's optimal effort selection is equivalent to finding a point at which the

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[10] Audit fee K in this case is actually an insurance fee. More precisely, it is the outside investors who are insured against investment in a low-type project. However, the firm's market price impounds such an insurance value of the audit, and it is passed to the owner. This is clear in equation (4), i.e., $V(\mathbf{q}) = \mu(\mathbf{q})(p_H R - I)$ if $D = \bar{D}$. In fact, any value of D greater than $\bar{D}$ overinsures investors and imposes punitive damages on the auditor. We rule out such a case in our model.

The dashed curve in Figure 3 is a locus of the equilibrium allocation $\mathbf{q}^*(D)$ for various levels of D. As D moves from zero to $\bar{D}$, $\mathbf{q}^*(D)$ travels in a *northwest* direction starting from E_1, passes through E_o when D equals D_o and then through E_a when D equals D_a, and finally reaches E_2. To understand this locus, suppose that the auditor's expected liability loss is arbitrarily close to zero. Then, from the auditor's problem in Section IV, we can see that the auditor's best response function becomes arbitrarily close to the horizontal axis (i.e., $q_a^r(q_o, D) \rightarrow 0$ for all q_o as $D \rightarrow 0$). On the other hand, when $D \rightarrow 0$, it is easy to verify that the owner's reaction function, $q_o^r(q_a, D)$, becomes identical to $q_o^F(q_a)$ for all q_a. Thus, as the auditor's expected liability loss becomes arbitrarily small, the equilibrium allocation converges to the point at which $q_o^F(q_a)$ intersects the horizontal axis, denoted by E_1 in Figure 3. Obviously, the auditor underinvests effort while the owner overinvests in ICS quality in this case.

From Proposition 1, we know that an increase in D motivates the auditor to provide more effort and induces the owner to reduce her investment in ICS quality. This is equivalent to saying that, as D increases, the equilibrium allocation $\mathbf{q}^*(D) = (q_o^*(D), q_a^*(D))$ moves in a northwest direction in (q_o, q_a) space. Therefore, as D increases from zero to D_o, the equilibrium allocation travels from E_1 to E_o, where E_o is the equilibrium allocation corresponding to D_o. That is, the owner's equilibrium investment in ICS quality is the same as the first-best level at E_o. Nonetheless, since $D_o < D_a$, the auditor's underinvestment problem persists. A further increase of D starting from D_o induces the owner to underinvest. However, unless D reaches D_a, the auditor continues underinvesting. As a result, when $D \in (D_o, D_a)$, the efficiency loss arises from both parties' underinvestments. When D becomes equal to D_a, the equilibrium allocation reaches E_a at which the auditor provides the first-best audit effort. However, since $D_a > D_o$, the owner's underinvestment problem becomes more severe. When the expected liability loss exceeds D_a, the equilibrium allocation continues its move in a northwest direction. Thus, the auditor begins to overinvest effort, while the owner's underinvestment problem becomes further exacerbated.

Now suppose that D becomes close to $\bar{D}$. In this case, the auditor's best response function becomes identical to $q_a^F(q_o)$ because the auditor fully internalizes the consequence of the negative NPV investment to a low-type project (see equation [11]). We also know that if $D = \bar{D}$, the owner has no incentive to make a positive investment in ICS quality. Hence, as $D \rightarrow \bar{D}$, the equilibrium allocation converges to the intersection of $q_a^F(q_o)$ and the vertical axis, denoted by E_2 in Figure 3. From the above discussion, one can see that the Stackelberg equilibrium in Figure 2, denoted by S, corresponds to an equilibrium allocation when $D \in (D_a, \bar{D})$, in which the auditor overinvests and the owner underinvests.

We now investigate how a change in D affects the equilibrium social surplus through its impact on the equilibrium allocation. Formally, the equilibrium social surplus is given by:

$$\Pi^S(\mathbf{q}^*(D)) = NPV_\lambda + (1 - \lambda)\phi(\mathbf{q}^*(D))(I - p_L R) - [q_o^*(D) + q_a^*(D)], \qquad (15)$$

which is identical to equation (7) except that we use the equilibrium allocation $\mathbf{q}^*(D) = (q_o^*(D), q_a^*(D))$ instead of $\mathbf{q}$. Differentiating $\Pi^S(\mathbf{q}^*(D))$ with respect to D yields:

$$\frac{d\Pi^S(\mathbf{q}^*(D))}{dD} = \left(\frac{\partial\Pi^S(\mathbf{q}^*(D))}{\partial q_o}\right)\left(\frac{dq_o^*(D)}{dD}\right) + \left(\frac{\partial\Pi^S(\mathbf{q}^*(D))}{\partial q_a}\right)\left(\frac{dq_a^*(D)}{dD}\right). \qquad (16)$$

Define:

$$\alpha(D) \equiv -\frac{\partial \Pi^S(\mathbf{q}^*(D))/\partial q_o}{\partial \Pi^S(\mathbf{q}^*(D))/\partial q_a} \quad \text{and} \quad \beta(D) \equiv \frac{dq_a^*(D)/dD}{dq_o^*(D)/dD}. \tag{17}$$

That is, $\alpha(D)$ is the slope of the iso-social-surplus curve at a given equilibrium allocation $\mathbf{q}^*(D)$. On the other hand, $\beta(D)$ is the ratio between the sensitivity of the equilibrium audit effort in response to an increase in D and that of the equilibrium ICS quality.

Proposition 3:

(i) Social surplus increases with D if, and only if, $\alpha(D)$ is greater than $\beta(D)$.

(ii) There exist two constants $D_1 \in (0, \bar{D})$ and $D_2 \in (0, \bar{D})$ where $D_1 \leq D_2$ such that social surplus increases with D for all $D < D_1$, while it decreases with D for all $D > D_2$.

(iii) If $\beta'(D) > 0$, then there exists a unique constant $D_s \in (0, \bar{D})$ such that social surplus attains its second-best maximum at $D = D_s$.

Part (i) states the necessary and sufficient condition under which an increase in the auditor's expected liability loss, D, results in an increase in the equilibrium social surplus. It shows that D's impact on social surplus solely depends on the relative size of $\alpha(D)$ and $\beta(D)$. To see why, consider the equilibrium allocation $S = \mathbf{q}^*(D)$ depicted in Figure 2. From Proposition 1, we know that $\beta(D)$ is negative, and thus, $\mathbf{q}^*(D)$ moves in a northwest direction when D increases. Such a move increases social surplus if, and only if, $\mathbf{q}^*(D)$ moves *inside* the iso-social-surplus curve passing though S. This occurs if, and only if, the iso-social-surplus curve at S is flatter than the locus of $\mathbf{q}^*(D)$. Since $\alpha(D)$ is also negative (see the proof in Appendix B), an increase in D improves social surplus if, and only if, $\alpha(D)$ is greater than $\beta(D)$.

Even though Part (i) is a general statement about the social welfare consequence of an increase in D in our model, note that the relative size of $\alpha(D)$ and $\beta(D)$ varies across different values of D. In contrast, Part (ii) shows that if D is sufficiently small (in the sense of $D < D_1$), an increase in D always improves social surplus (i.e., $\alpha(D) > \beta(D)$ for all $D < D_1$), and the converse is true if D is sufficiently large (i.e., $\alpha(D) < \beta(D)$ for all $D > D_2$). To explain the intuition, suppose that D is sufficiently close to zero. In this case, the auditor severely underinvests effort while the owner severely overinvests in ICS quality. Thus, an increase in D results in an increase in audit effort and a decrease in ICS quality. This alleviates the misallocation problem, thereby improving social surplus. On the other hand, when D is sufficiently large, say, close to $\bar{D}$, the efficiency loss arises from the owner's underinvestment and the auditor's overinvestment. An increase in D in this situation strengthens the owner's underinvestment incentive and at the same time induces the auditor to provide more effort, thereby exacerbating the misallocation problem and further lowering social surplus.[15]

Part (iii) states a sufficient condition under which the equilibrium social surplus given in equation (15) attains its maximum at a unique value $D_s \in (0, \bar{D})$. That is, $\Pi^S(\mathbf{q}^*(D_s))$

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[15] In general, D_1 and D_2 in Proposition 3 are not directly comparable to D_o and D_a in Proposition 2. As shown in the proof, D_1 and D_2 are the values of D at which $\Pi^{S\prime}(\mathbf{q}^*(D)) = 0$, i.e., $\alpha(D) = \beta(D)$. Such values of D have nothing to do with values of D that induce either the owner's or the auditor's equilibrium choice to be the same as his or her first-best choice, i.e., D_o and D_a. For example, at $D = D_o$, $\alpha(D_o)$ might be greater or less than $\beta(D_o)$.

$\geq \Pi^S(\mathbf{q}^*(D))$ for all $D \in (0, \bar{D})$. This is equivalent to saying that, when $\beta'(D) > 0$, D_1 and D_2 in Part (ii) shrink to a single point ($D_1 = D_2 \equiv D_s$) so that the equilibrium social surplus is increasing in D if, and only if, $D < D_s$. To explain the condition $\beta'(D) > 0$, we return to Figure 3. Given that there is a one-to-one correspondence between D and any point on the locus of the equilibrium allocation (i.e., the dashed curve in Figure 3), one can find a social-welfare-maximizing D by locating an allocation on the locus at which social surplus is maximized. However, as evident in Figure 3, such an allocation may not be unique in general. Nonetheless, if $\beta'(D) > 0$, then one can verify that the locus of the equilibrium allocation must be concave in (q_o, q_a) space (*unlike* the locus depicted in Figure 3). If so, the locus is tangential to an iso-social-surplus curve at a unique point, say, $\mathbf{q}^s \equiv (q_o^s, q_a^s)$, because Π^S is a strictly concave function of $\mathbf{q}$. The value of D that corresponds to $\mathbf{q}^s$ is D_s.[16] Here, we must emphasize that D_s is the second-best optimum in the sense that the equilibrium social surplus given D_s is strictly less than the first-best social surplus; that is, $\Pi^S(\mathbf{q}^*(D_s)) < \Pi^S(\mathbf{q}^\dagger)$. This is because, as shown in Corollary 1, an efficiency loss from the misallocation of ICS quality and audit effort is inevitable in the second-best case for any given D.[17]

Some Regulatory Implications of the Analysis

We conclude this section by interpreting recent changes in auditors' legal environment in the context of our model. Corollary 1 shows that if legal liability is excessive (as often claimed by practicing auditors, e.g., O'Malley [1993] and Weinback [1993]), an efficiency loss arises from the owner's underinvestment in ICS quality and the auditor's overprovision of effort. The SEC's recent emphasis on strengthening firms' internal controls[18] would ameliorate any effects of excessive legal liability on firms' disincentive to invest in internal controls and auditors' overprovision of effort, thereby achieving a better allocation of resources.

Second, Proposition 3 suggests that *if* auditors' legal liability was excessive, then the recent Private Securities Litigation Reform Act of 1995 (the Reform Act) might enhance social efficiency. The Reform Act curbs financial statement users' litigation against auditors, thereby reducing auditors' expected liability loss. In light of Part (ii) of Proposition 3, *if* auditors' legal liability prior to the Reform Act was sufficiently high, then lowering auditors' expected liability loss (i.e., a decrease in D) improves social surplus by *simultaneously* alleviating the misallocation of firms' investment in ICS quality and auditors' effort provision.

Nevertheless, insofar as an efficiency loss arises from two different sources (i.e., firms' and auditors' private incentives), our analysis also illustrates that regulators cannot completely eliminate the efficiency loss by simply changing auditors' legal liability. To elaborate, recall that D_s given in Part (iii) of Proposition 3 is the second-best optimal liability level; i.e., it never achieves the first-best social surplus. The *dual* source of incentive problems is crucial to this result. To be more precise, suppose that a regulator can directly enforce a certain level of q_o, and that only the audit effort is uncontrollable. If so, the regulator can achieve the first-best outcome by enforcing $q_o = q_o^\dagger$ *and* setting $D = \bar{D}$. This is because, when $D = \bar{D}$, the auditor's best response function becomes identical to the coordination function in the first-best case (see equation [11]). Given that ICS quality is equal to the first-best level $q_o^\dagger$, the auditor's optimal effort investment is $q_a^\dagger$, and hence, the first-best allocation $\mathbf{q}^\dagger$ is achieved.[19] However, absent regulatory intervention in the owner's ICS quality choice, setting $D = \bar{D}$ induces the owner to choose zero ICS quality, which in turn leads to an extreme overprovision of audit effort. Therefore, as Proposition 3 shows, the optimal audit liability in the second-best case must be strictly lower than the liability level that induces the auditor to fully internalize a wasteful investment in a low-type project (i.e., $D_s < \bar{D}$).

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[16] Using the definition of $\beta(D)$, one can check that $\beta'(D) > 0$ if both $q_o^*(D)$ and $q_a^*(D)$ are concave in D. In fact, this concavity condition implies that $\Pi^S(\mathbf{q}^*(D))$ is concave in D so that, given the result in Part (ii), $\Pi^S(\mathbf{q}^*(D))$ attains a unique interior maximum.

[17] Thus far, our analysis has been based on the assumption of $\phi_{ao} < 0$, which implies that ICS quality and audit effort are strategic substitutes. If one alternatively assumes $\phi_{ao} > 0$ (in which case q_o and q_a exhibit strategic complementarity in the sense of Bulow et al. [1985]), then some results in Propositions 1 and 2 change. For example, the audit effort may increase or decrease as the legal liability increases. Also, even if the first-best outcome is still unattainable, the efficiency loss in this case arises from both parties' underinvestments; i.e., $q_o^*(D) < q_o^\dagger$ and $q_a^*(D) < q_a^\dagger$ for any $D \in (0, \bar{D})$. However, one can verify that Proposition 3 remains unchanged under the alternative assumption $\phi_{ao} > 0$; that is, the results in Proposition 3 are robust against the sign of the cross partial derivative of ϕ. A formal proof of the above arguments is available from the authors upon request.

[18] For example, the SEC called for improving the effectiveness of corporate audit committees, which constitute a key element of internal controls (see the report of the Blue Ribbon Committee on Improving the Effectiveness of Corporate Audit Committees [BRC 1999]).

Our result that a regulatory agency cannot achieve the first-best outcome by using a single regulatory instrument D contrasts with Schwartz's (1997) result that, under the strict liability rule, the first-best outcome (in her model) is attainable if a regulator chooses an appropriate damage award. This difference arises because we focus on different incentive problems. Schwartz (1997) considers an auditor's incentive problem associated with effort provision and outside investors' incentive problem associated with their capital investment. On the other hand, our paper addresses the owner's and the auditor's incentive problems associated with their joint production of information. In Schwartz's (1997) model, the regulator can eliminate the outside investors' incentive problem by making their potential *ex post* damage recovery *independent* of their *ex ante* capital investment. Thus, the regulator's remaining concern is to choose an appropriate damage award (independent of the investors' actual investment) that induces the auditor to provide the first-best effort. Schwartz (1997) shows that there exists such a damage award in her model. However, in our model, no damage award can eliminate both the owner's and the auditor's incentive problems in their joint production of information, because any damage award has an insurance effect on the owner's payoff. This insurance effect distorts the owner's investment in ICS quality, which in turn affects the auditor's effort decision.[20]

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[19] This may not be the only way to achieve the first-best allocation. That is, other regulatory policies may lead to the first-best outcome. For example, decoupling audit liability (à la Polinsky and Che 1991) may be an alternative. While it may be difficult—if not impossible—to enforce a particular level of internal control system quality, our main point is that to completely eliminate the efficiency loss, regulators must control both the owner's and the auditor's incentive problems simultaneously. This requires at least two regulatory instruments.

[20] Although we simplify the model by assuming that outside investors' capital investment is a fixed amount I, our result generalizes to a setting where the investors' capital investment is endogenous. To be specific, to make our model directly comparable to Schwartz (1997), suppose $p_L = 0$ and let outside investors choose any level of $I \geq 0$. Also, let R(I) be the return from investing I in a high-type project where R is an increasing and concave function of I. Since we assume an exogenous damage award D (i.e., independent of I) under the strict liability regime, it follows from Schwartz (1997) that investors fully internalize the consequences of investment I; i.e., their optimal choice of I is the same as the socially optimal one. Then, the question is whether it is possible for a regulator to choose an arbitrary level of damage award that induces *both* the owner *and* the auditor to choose the socially optimal levels of internal control system quality and audit effort, respectively. This paper shows that there does *not* exist such a damage award. More precisely, note that the market value of the firm in this case consists of the investors' optimized expected return from the project and their expected liability recovery. Then, using the fact that both components translate into the owner's payoff function, one can verify that $(1 - \lambda)(1 - \phi(\mathbf{q}))D$ is the owner's *ex ante* benefit from the audit's insurance value. Since q_o and D are *multiplicatively* separable, D affects the owner's optimal choice of q_o in the manner we have analyzed in Section IV.

VI. SUMMARY

This paper presents a model of imperfect auditing in which a firm's owner, an auditor, and outside investors strategically interact. An audit benefits the owner because it increases the *ex ante* market value of the firm by detecting an unprofitable project. The distinguishing feature of our study is that the informativeness of audit reports depends not only on the auditor's effort, but also on the owner's costly investment in the internal control system. We show that if the auditor's expected liability loss is sufficiently large, the owner underinvests in the internal control system and the auditor overinvests effort, leading to an efficiency loss. While reducing the auditor's legal liability in this situation improves social efficiency by alleviating the misallocation of the owner's and auditor's investments, insofar as the owner's and auditor's incentive problems arise from their joint production of information, regulators cannot completely eliminate the efficiency loss by changing legal liability alone.

Recent changes in the regulatory environment can be interpreted in the context of our model. Our analysis suggests that *if* auditors' legal liability was excessive, then the Private Securities Litigation Reform Act of 1995 (which reduces auditors' legal liability) may enhance social efficiency. In addition, the SEC's recent effort to enhance corporate internal controls may be beneficial *if* excessive auditors' legal liability has led firms to underinvest in their internal control systems.

APPENDIX A
NOTATION

I	Required capital investment to undertake a project.
R	Return in the case of project success.
$t \in \{L, H\}$	Type of project, where L represents a low type and H represents a high type.
λ	Common prior probability that the project type is high ($t = H$).
p_t	The probability that type-t project succeeds.
NPV_λ	Expected return (net present value) of a project given λ.
$\{\ell, h\}$	Set of audit reports on the project type, where ℓ (h) represents the auditor's attestation that the project type is low (high).
$\mathbf{q} \equiv (q_o, q_a)$	Pair of the firm's internal control system (ICS) quality, q_o, and the auditor's effort, q_a.
$\phi(\mathbf{q})$	The probability that audit report ℓ is correctly issued to a type-L project given $\mathbf{q}$.
$D \in (0, \bar{D})$	Damage award measured by the investors' (auditor's) expected gain (loss) in the case of an audit failure where $\bar{D} \equiv (I - p_L R)/(1 - p_L)$.
$\mu(\mathbf{q})$	Investors' posterior beliefs that the project type is H given audit report h.
$V(\mathbf{q})$	Competitive market value of the firm given audit report h.
$\Pi^j(\mathbf{q})$	The owner's ($j = O$) and auditor's ($j = A$) *ex ante* expected payoff given $\mathbf{q}$.
$\Pi^S(\mathbf{q})$	Social surplus given $\mathbf{q}$.
$q_i^F(q_j)$	Coordination function, i.e., the first-best q_i given q_j for $i \neq j \in \{o, a\}$.
$\mathbf{q}^\dagger \equiv (q_o^\dagger, q_a^\dagger)$	The first-best allocation of $\mathbf{q}$.
$q_a^r(q_o, D)$	Auditor's best response to q_o given D.
$q_o^r(q_a, D)$	Owner's best response to q_a given D *if* the auditor does *not* observe q_o.
$q_i^*(D)$	The equilibrium internal control system quality ($i = o$) and audit effort ($i = a$).

$\mathbf{q}^*(D)$	The equilibrium allocation of $\mathbf{q}$ given D, i.e., $\mathbf{q}^*(D) \equiv (q_o^*(D), q_a^*(D))$.
D_i	The value of D that induces the first-best ICS quality ($i = o$) and the first-best audit effort ($i = a$).
$\alpha(D)$	The slope of the iso-social-surplus curve at a given equilibrium $\mathbf{q}^*(D)$.
$\beta(D)$	The rate of change in the equilibrium audit effort relative to that of the equilibrium internal control system quality when D increases.
D_1	The value of D, below which social surplus increases with D.
D_2	The value of D, above which social surplus decreases with D.
D_s	The value of D, at which the equilibrium (i.e., second-best) social surplus attains its maximum.

APPENDIX B
PROOFS

Proof of Proposition 1

To simplify notation, define:

$$\begin{aligned} f(q_o, D) &\equiv \frac{d}{dq_o}\,[\Pi^O(q_o, q_a^r(q_o, D), D)] \\ &= (1-\lambda)[(I - p_L R) - (1 - p_L)D] \\ &\quad\times \left(\phi_o(q_o, q_a^r(q_o, D)) + \phi_a(q_o, q_a^r(q_o, D))\,\frac{\partial q_a^r(q_o, D)}{\partial q_o}\right) - 1. \end{aligned} \tag{B.1}$$

In equilibrium, equation (14) is equivalent to $f(q_o^*(D), D) = 0$. Totally differentiating $f(q_o^*(D), D) = 0$ with respect to D yields:

$$\frac{\partial f}{\partial q_o}\frac{dq_o^*}{dD} + \frac{\partial f}{\partial D} = 0.$$

The second-order condition implies that $\partial f/\partial q_o = (d^2/dq_o^2)\,[\Pi^O(q_o, q_a^r(q_o, D), D)] < 0$. Thus, to show $dq_o^*/dD < 0$, we only need to show that $\partial f/\partial D < 0$.

From equation (B.1), we have:

$$\begin{aligned} \frac{\partial f}{\partial D} &= -(1-\lambda)(1-p_L)\left(\phi_o + \phi_a\frac{\partial q_a^r}{\partial q_o}\right) \\ &\quad + (1-\lambda)[(I - p_L R) - (1-p_L)D]\left(\phi_{oa}\frac{\partial q_a^r}{\partial D} + \phi_{aa}\frac{\partial q_a^r}{\partial D}\frac{\partial q_a^r}{\partial q_o}\right) \\ &\quad + (1-\lambda)[(I - p_L R) - (1-p_L)D]\phi_a\frac{\partial^2 q_a^r}{\partial D\partial q_o}. \end{aligned} \tag{B.2}$$

Since $D < \bar{D}$, equation (14) implies $\phi_o + \phi_a(\partial q_a^r/\partial q_o) > 0$. Hence, the first term in equation (B.2) is strictly negative. Next, by differentiating equation (10) with respect to q_o and D, we have $\partial q_a^r/\partial q_o = -\phi_{oa}/\phi_{aa} < 0$ and $\partial q_a^r/\partial D = -\phi_a/D\phi_{aa} > 0$, respectively. As a result, the second term in equation (B.2) is equal to 0. It thus follows that if $\partial^2 q_a^r/\partial D\partial q_o \le 0$ in equation (B.2), then $\partial f/\partial D < 0$, which in turn implies $dq_o^*/dD < 0$.

Next, consider the equilibrium audit effort. Since $q_a^*(D) \equiv q_a^r(q_o^*(D), D)$, we have:

$$\frac{dq_a^*}{dD} = \frac{\partial q_a^r}{\partial q_o}\frac{dq_o^*}{dD} + \frac{\partial q_a^r}{\partial D} > 0,$$

where the inequality follows from the fact that $\partial q_a^r/\partial q_o < 0$, $dq_o^*/dD < 0$, and $\partial q_a^r/\partial D > 0$. ■

Proof of Proposition 2

Let $D \rightarrow 0$. Then, since $q_a^r(q_o, D) \rightarrow 0$ for all q_o, it must be true that $\partial q_a^r/\partial q_o \rightarrow 0$ for all q_o. From equation (14), this implies that $q_o^*(D) \rightarrow \bar{q}_o$ as $D \rightarrow 0$, where $\bar{q}_o$ is defined by:

$$(1 - \lambda)(I - p_L R)\phi_o(\bar{q}_o, 0) = 1. \tag{B.3}$$

From equation (8), we also have:

$$(1 - \lambda)(I - p_L R)\phi_o(q_o^\dagger, q_a^\dagger) = 1. \tag{B.4}$$

Then, equation (B.3) and equation (B.4) imply that:

$$\phi_o(\bar{q}_o, 0) = \phi_o(q_o^\dagger, q_a^\dagger) < \phi_o(q_o^\dagger, 0),$$

where the inequality follows from $\phi_{oa} < 0$ and $q_a^\dagger > 0$. Given that $\phi_{oo} < 0$ and $\phi_o(\bar{q}_o, 0) < \phi_o(q_o^\dagger, 0)$, we have:

$$\lim_{D\rightarrow 0} q_o^*(D) = \bar{q}_o > q_o^\dagger. \tag{B.5}$$

Similarly, let $D \rightarrow \bar{D}$. Then, we know $q_o^*(D) \rightarrow 0$, which in conjunction with equation (10) implies that $q_a^*(D) \rightarrow \bar{q}_a$, where $\bar{q}_a$ is defined by:

$$(1 - \lambda)(I - p_L R)\phi_a(0, \bar{q}_a) = 1. \tag{B.6}$$

From equation (9), we have:

$$(1 - \lambda)(I - p_L R)\phi_a(q_o^\dagger, q_a^\dagger) = 1. \tag{B.7}$$

Then, using equation (B.6) and equation (B.7), we must have:

$$\phi_a(0, \bar{q}_a) = \phi_a(q_o^\dagger, q_a^\dagger) < \phi_a(0, q_a^\dagger),$$

where the inequality follows from $\phi_{oa} < 0$ and $q_o^\dagger > 0$. Given that $\phi_{aa} < 0$ and $\phi_a(0, \bar{q}_a) < \phi_a(0, q_a^\dagger)$, we have:

$$\lim_{D\rightarrow \bar{D}} q_a^*(D) = \bar{q}_a > q_a^\dagger. \tag{B.8}$$

Proposition 1 shows that $dq_o^*(D)/dD < 0$ for all $D \in (0, \bar{D})$. Given equation (B.5) and the fact that $q_o^*(D) \rightarrow 0$ as $D \rightarrow \bar{D}$, it follows from the intermediate value theorem that there exists a unique constant $D_o \in (0, \bar{D})$ such that $q_o^*(D_o) = q_o^\dagger$ and $q_o^*(D) > q_o^\dagger$ if, and only if, $D < D_o$. Similarly, using the intermediate value theorem together with $dq_a^*(D)/dD > 0$ in Proposition 1, equation (B.8), and the fact that $q_a^*(D) = q_a^r(q_o^*(D), D) \to 0$ as $D \to 0$, we conclude that there exists a unique constant $D_a \in (0, \bar{D})$ such that $q_a^*(D_a) = q_a^\dagger$ and $q_a^*(D) > q_a^\dagger$ if, and only if, $D > D_a$.

It remains to show that $D_o < D_a$. Consider the auditor's first-order condition (10) in equilibrium. When $D = D_o$, we must have:

$$(1 - \lambda)(1 - p_L)D_o\phi_a(q_o^\dagger, q_a^*(D_o)) = 1, \tag{B.9}$$

where we use $q_o^*(D_o) = q_o^\dagger$. Since $D_o < \bar{D}$, equation (B.7) and equation (B.9) imply that:

$$\phi_a(q_o^\dagger, q_a^*(D_o)) > \phi_a(q_o^\dagger, q_a^\dagger).$$

Since $\phi_{aa} < 0$, the above inequality implies that $q_a^*(D_o) < q_a^\dagger$. Now suppose that $D_o \geq D_a$. Then, since $q_a^*(D)$ is strictly increasing in D, we must have $q_a^*(D_o) \geq q_a^*(D_a) = q_a^\dagger$, where the last equality follows from the property of D_a. However, this is a contradiction to $q_a^*(D_o) < q_a^\dagger$. This completes the proof of Proposition 2. ■

Proof of Corollary 1

Consider any $D \in (0, D_o]$. From Proposition 2, we know that $q_o^*(D_o) = q_o^\dagger$. Also, from Proposition 1, we have $dq_o^*(D)/dD < 0$ for all D. Hence, $q_o^*(D) \geq q_o^\dagger$ for all $D \in (0, D_o]$. On the other hand, since $q_a^*(D_a) = q_a^\dagger$, $D_o < D_a$ (Proposition 2), and $dq_a^*(D)/dD > 0$ for all D (Proposition 1), it follows that $q_a^*(D) < q_a^\dagger$ for all $D \in (0, D_o]$. This completes the comparison of $\mathbf{q}^*(D)$ with $\mathbf{q}^\dagger$ when $D \in (0, D_o]$. We omit the proof of the remaining cases because it is similar to the case of $D \in (0, D_o]$. ■

Proof of Proposition 3

(i) From equation (16), note that:

$$\frac{d\Pi^S(\mathbf{q}^*(D))}{dD} = \left(\frac{\partial\Pi^S(\mathbf{q}^*(D))}{\partial q_o}\right)\left(\frac{dq_o^*(D)}{dD}\right)\left[1 - \frac{\beta(D)}{\alpha(D)}\right], \tag{B.10}$$

where $\alpha(D)$ and $\beta(D)$ are defined in equation (17). We know that $dq_o^*(D)/dD < 0$. In addition:

$$\begin{aligned}
&\frac{\partial\Pi^S(\mathbf{q}^*(D))}{\partial q_o}\\
&= (1 - \lambda)(1 - p_L R)\phi_o - 1\\
&= (1 - \lambda)(1 - p_L R)\phi_o - (1 - \lambda)[(1 - p_L R) - (1 - p_L)D]\left(\phi_o + \phi_a\frac{\partial q_a^r}{\partial q_o}\right)\\
&= (1 - \lambda)(1 - p_L)D\phi_o - (1 - \lambda)[(1 - p_L R) - (1 - p_L)D]\phi_a\frac{\partial q_a^r}{\partial q_o} > 0,
\end{aligned} \tag{B.11}$$

where we use equation (14) in the second equality and the last inequality is due to the fact that $\phi_o > 0$, $\phi_a > 0$, $\partial q_a^r/\partial q_o < 0$, and $D \in (0, \bar{D})$. Thus, from equation (B.10), we have:

$$\frac{d\Pi^S}{dD} > 0 \Leftrightarrow 1 < \frac{\beta(D)}{\alpha(D)}. \tag{B.12}$$

From Proposition 1, we know that $\beta(D)$ is negative for all $D \in (0, \bar{D})$. On the other hand, note that:

$$\begin{aligned}\frac{\partial \Pi^S(\mathbf{q}^*(D))}{\partial q_a} &= (1 - \lambda)(I - p_L R)\phi_a - 1 \\ &= (1 - \lambda)(I - p_L R)\phi_a - (1 - \lambda)(1 - p_L)D\phi_a > 0, \end{aligned} \tag{B.13}$$

where we use Equation (10) in the second equality and the last inequality is due to $D < \bar{D}$. Equations (B.11) and (B.13) thus imply that $\alpha(D)$ is also negative for all $D \in (0, \bar{D})$. Rearranging the second inequality in (B.12) establishes the result.

(ii) First, let $D \rightarrow 0$. Using $\partial q_a^r/\partial q_o \rightarrow 0$ and $\phi_a \rightarrow \infty$ (since $q_a^* \rightarrow 0$ as $D \rightarrow 0$), it is easy to verify that $\partial\Pi^S(\mathbf{q}^*(D))/\partial q_o \rightarrow 0$ in equation (B.11) and $\partial\Pi^S(\mathbf{q}^*(D))/\partial q_a \rightarrow \infty$ in equation (B.13). On the other hand, observe that:

$$\frac{dq_a^*}{dD} = \frac{\partial q_a^r}{\partial q_o}\frac{dq_o^*}{dD} + \frac{\partial q_a^r}{\partial D} > \frac{\partial q_a^r}{\partial D} = -\frac{\phi_a}{D\phi_{aa}} \rightarrow \infty \text{ as } D \rightarrow 0.$$

Hence, using equation (16), we conclude that $d\Pi^S(\mathbf{q}^*(D))/dD \rightarrow \infty$ as $D \rightarrow 0$.

Second, let $D \rightarrow \bar{D}$. Then, it is easy to verify that $\partial\Pi^S(\mathbf{q}^*(D))/\partial q_o \rightarrow \infty$ in equation (B.11) (since $q_o^* \rightarrow 0$ implies $\phi_o \rightarrow \infty$) and $\partial\Pi^S(\mathbf{q}^*(D))/\partial q_a \rightarrow 0$ in equation (B.13), respectively. In addition, from the proof of Proposition 1, we know $dq_o^*/dD = -(\partial f/\partial D)/(\partial f/\partial q_o)$. Using equation (B.2), one can also check that $\partial f/\partial D \rightarrow -\infty$ and $\partial f/\partial q_o = d^2/dq_o^2\,[\Pi^O(q_o, q_a^r(q_o, D), D)] \rightarrow 0$ from below (recall that $\partial f/\partial q_o < 0$ due to the second-order condition). As a result, we have $dq_o^*/dD \rightarrow -\infty$ and thus $d\Pi^S(\mathbf{q}^*(D))/dD \rightarrow -\infty$ as $D \rightarrow \bar{D}$.

Finally, given that $d\Pi^S(\mathbf{q}^*(D))/dD \rightarrow \infty$ as $D \rightarrow 0$ whereas $d\Pi^S(\mathbf{q}^*(D))/dD \rightarrow -\infty$ as $D \rightarrow \bar{D}$, the intermediate value theorem dictates that there exist $D_1 \in (0, \bar{D})$ and $D_2 \in (0, \bar{D})$ where $D_1 \leq D_2$ such that $d\Pi^S(\mathbf{q}^*(D))/dD > 0$ for all $D < D_1$, while $d\Pi^S(\mathbf{q}^*(D))/dD < 0$ for all $D > D_2$.

(iii) For any given D, define a compact set $\Gamma(D) \subset \Re_+^2$ such that:

$$\Gamma(D) \equiv \{(q_o, q_a) \mid q_o \leq q_o^*(D) \text{ and } q_a \leq q_a^*(D)\}.$$

Consider the following optimization problem:

$$\max_{q_o, q_a} \Pi^S(\mathbf{q}) \text{ subject to } \mathbf{q} \in \Gamma \equiv \bigcup_{D \in [0, \bar{D}]} \Gamma(D).$$

Note that if $\beta'(D) > 0$ for all D, then Γ is a convex subset of $\Re^2_+$. This, in conjunction with the fact that Π^S is a strictly concave function of $\mathbf{q}$, implies that there exists a unique optimum, say, $\mathbf{q}^s$, at the frontier of Γ. Now observe that the frontier of Γ and D has a one-to-one correspondence. The proof of Proposition 3 (ii) shows that the optimum cannot be an allocation that corresponds to either $\underline{D} = 0$ or $\bar{D}$. Hence, $\mathbf{q}^s$ must be an allocation corresponding to a value of $D \in (0, \bar{D})$. Denoting such a value of D as D_s completes the proof. ■

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The Mispricing of Abnormal Accruals

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ABSTRACT: This paper examines the market pricing of Jones (1991) model-estimated abnormal accruals (often termed "discretionary accruals" in the prior literature) to test whether stock prices rationally reflect the one-year-ahead earnings implications of these accruals. Using the Mishkin (1983) and hedge-portfolio test methods Sloan (1996) employs, I find that the market overestimates the persistence, or one-year-ahead earnings implications, of abnormal accruals, and consequently overprices these accruals. These results extend Subramanyam (1996) by demonstrating that the market not only prices, but also overprices abnormal accruals. They also suggest that the overpricing of total accruals that Sloan (1996) documents is due largely to abnormal accruals. The results are robust to five alternative measures of abnormal accruals, and still hold when I estimate abnormal accruals after controlling for major unusual but largely nondiscretionary accruals. The latter finding is consistent with the notion that the market overprices the portion of abnormal accruals stemming from managerial discretion.

Keywords: *accruals; abnormal accruals; discretionary accruals; mispricing.*

Data Availability: *Data used in this study are available from public sources identified in the study.*

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I. INTRODUCTION

This paper examines the market pricing of Jones (1991) model-estimated abnormal accruals (often termed "discretionary accruals" in the prior literature) to test whether stock prices rationally reflect the one-year-ahead earnings implications of these accruals. Subramanyam (1996) finds that the market prices Jones (1991) model-estimated abnormal accruals, and that abnormal accruals are positively associated with future profitability. However, Subramanyam's (1996) evidence that abnormal accruals are positively related to future profitability does not necessarily mean that the market *rationally* prices these accruals with respect to their association with future profitability, such as earnings.

Sloan (1996) investigates the market pricing of *total* accruals. He finds that the market fails to appreciate fully the lower persistence of the accrual component of earnings and, consequently, overprices total accruals. Using quarterly data, Collins and Hribar (2000a) also find that the market overprices total accruals. Neither Sloan (1996) nor Collins and Hribar (2000a) investigate whether the overpricing is due to abnormal accruals, normal accruals, or both.

Teoh et al. (1998a, 1998b) and Rangan (1998) document that managers choose positive abnormal accruals to opportunistically increase earnings before initial public offerings (IPOs) or seasoned equity offerings, and that the market overprices these abnormal accruals. However, the extant literature has not investigated whether the market misprices abnormal accruals in more general settings where managers may or may not have opportunistic incentives to manipulate earnings. This paper empirically examines this issue.

Following Sloan (1996), I use the Mishkin (1983) test and the hedge-portfolio test methods to examine whether the market rationally prices abnormal accruals with respect to their one-year-ahead earnings implications. The Mishkin (1983) test provides a statistical comparison between: (1) a measure of the market's pricing of abnormal accruals (i.e., the market's *valuation coefficient* on abnormal accruals) and (2) a measure of abnormal accruals' ability to predict one-year-ahead earnings (i.e., the *forecasting coefficient* of these accruals). If the market's valuation coefficient on abnormal accruals is significantly larger than the forecasting coefficient of these accruals for one-year-ahead earnings, then the Mishkin (1983) test would indicate that the market overprices abnormal accruals. Conversely, if the valuation coefficient is significantly smaller than the forecasting coefficient, then the Mishkin (1983) test would suggest that the market underprices abnormal accruals. Since the forecasting coefficient is a measure of the persistence of abnormal accruals (Freeman et al. 1982; Sloan 1996), I attribute any market mispricing of abnormal accruals to the market's failure to correctly assess the persistence of these accruals.

The hedge-portfolio test forms a portfolio long in firms in the most negative decile and short in firms in the most positive decile of current abnormal accruals. Evidence that the hedge portfolio yields consistently positive abnormal returns in subsequent years would suggest that the market overprices abnormal accruals in the portfolio formation year.

My Mishkin (1983) test results suggest that abnormal accruals are less persistent than normal accruals, which, in turn, are less persistent than cash from operations. Moreover, the market overestimates the persistence of, and thus overprices, both abnormal and normal accruals, although the overpricing of abnormal accruals is more severe. The hedge-portfolio test results confirm the overpricing of abnormal accruals detected by the Mishkin (1983) test, but do not support the overpricing of normal accruals. Taken together, the results of these two tests suggest that the market overprices abnormal accruals, but that it does not materially misprice normal accruals.

Prior research has often termed the Jones (1991) model-estimated abnormal accruals as "discretionary accruals," and then used these abnormal accruals as a proxy for managerial discretion (e.g., Jones 1991; Subramanyam 1996; Erickson and Wang 1999). However, Healy (1996) and Bernard and Skinner (1996) point out that the Jones (1991) model (hereafter the Jones model) residuals capture not only managerial discretion, but also unusual nondiscretionary accruals and unintentional misstatements. Due to this measurement error in the Jones model residuals, it is difficult to ascertain whether the market overprices that portion of abnormal accruals stemming from earnings management or the portion arising from unusual business circumstances. To provide evidence on this issue, I estimate abnormal accruals after controlling for major unusual accruals and non-articulation events (i.e., mergers, acquisitions, and divestitures) following the suggestions in Bernard and Skinner (1996) and Collins and Hribar (2000b). I find that my refined measure of abnormal accruals, which more cleanly isolates managerial discretion, is still overpriced.

This paper contributes to the literature on abnormal accruals and market efficiency in several ways. First, it provides direct evidence that the market overprices abnormal accruals. Besides extending Subramanyam (1996) and Sloan (1996), this finding adds to a growing body of research suggesting that the market does not fully impound publicly disclosed accounting information (Freeman and Tse 1989; Bernard and Thomas 1989, 1990; Sloan 1996; Teoh et al. 1998a, 1998b; Rangan 1998; Collins and Hribar 2000a). My results are also consistent with the conclusion reached in a contemporaneous and independently developed study by DeFond and Park (2001). Their conclusion that the market overprices abnormal accruals because investors underanticipate the future reversal of these accruals is entirely consistent with my conclusion that the market overprices abnormal accruals because investors overestimate the persistence of these accruals. My study, however, differs from DeFond and Park (2001) in terms of research design, research question, and controls for measurement error in the balance-sheet-estimated total accruals.[1]

Second, the evidence in this paper suggests that the market mispricing of abnormal accruals is due to its inability to correctly assess the persistence of abnormal accruals. Moreover, this mispricing is not limited to settings that give managers opportunistic incentives to manipulate earnings, such as before IPOs or seasoned equity offerings. This suggests that investors may benefit from disclosures that help them assess the persistence of accrual information. Hribar's (2000) finding that the market does not materially misprice those transitory accrual items that financial reports make relatively visible, such as special item accruals and LIFO liquidations, is consistent with the notion that appropriate disclosures may help the market correctly assess the persistence of accrual information.

Finally, I control for major unusual accruals and non-articulation events when estimating abnormal accruals. Evidence that the market still overprices this refined measure of abnormal accruals is consistent with the market overpricing the portion of abnormal accruals arising from managerial discretion.

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[1] For example, I use a long-window design while DeFond and Park (2001) use a short-window design and a different abnormal accrual model. I examine whether the market assigns an appropriate valuation coefficient to abnormal accruals. DeFond and Park (2001), on the other hand, investigate whether the market valuation of earnings surprises (i.e., the earnings response coefficient or ERC) for good and bad news firms, respectively, differs across income-increasing and income-decreasing abnormal accruals, and whether the market fully anticipates the future reversal of abnormal accruals. Finally, I control for measurement error in the balance-sheet-estimated total accruals following Collins and Hribar (2000b) in an attempt to better isolate the portion of abnormal accruals stemming from managerial discretion, whereas they do not.

The paper proceeds as follows. Section II describes the sample and variable measurement, Section III provides evidence on the market overpricing of abnormal accruals, and Section IV conducts sensitivity analyses. I conclude the paper in Section V.

II. RESEARCH DESIGN

Sample Selection

I obtain all data from the 1995 Compustat Annual Industrial, Research, and Full Coverage files, and the 1995 CRSP monthly return files. Since funds from operations (Compustat item #110) are not available until 1971 and since the analysis requires stock returns for three years after the sample year, my sample covers the 22-year period from 1971 to 1992.[2] For this sample period, I delete firm-year observations with: (1) missing beginning-of-year total assets or insufficient data to calculate accruals as defined below; (2) NASDAQ observations prior to 1982;[3] (3) missing monthly stock returns on the 1995 CRSP files; and (4) fewer than six observations in any two-digit SIC code and year combination, or where any variable in the Jones model (described below) has a value that is more than three standard deviations away from its mean. The final sample consists of 7,506 firms and 56,692 firm-year observations from 1971 to 1992.

Variable Measurement

I use Subramanyam's (1996) definitions of earnings, accruals, and cash from operations. Earnings ($EARN_t$) are defined as income before extraordinary items (Compustat item #18), and cash from operations (CFO_t) is net cash flows from operating activities reported under SFAS No. 95 (Compustat item #308). For firm-years prior to 1988 when Compustat item #308 is unavailable, I estimate CFO_t as follows:

$$CFO_t = FFO_t - \Delta CA_t + \Delta CASH_t + \Delta CL_t - \Delta STDEBT_t,$$

where:

FFO_t = funds from operations (Compustat item #110);
ΔCA_t = change in current assets (Compustat item #4);
$\Delta CASH_t$ = change in cash and short-term investments (Compustat item #1);
ΔCL_t = change in current liabilities (Compustat item #5); and
$\Delta STDEBT_t$ = change in short term debt (Compustat item #34).

Total accruals ($ACCR_t$) are measured as the difference between earnings and cash from operations; i.e., $ACCR_t = EARN_t - CFO_t$.[4] All three variables are deflated by beginning-of-year total assets (TA_{t-1}, Compustat item #6).

I use the Jones model to estimate normal accruals and abnormal accruals (firm subscript is omitted for ease of exposition):

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[2] To avoid a survivorship bias, my sample contains firms that cease to exist after the sample year. For these nonsurviving firms, return and Compustat data are missing in one or more years after the sample year.

[3] I use size-adjusted returns defined as the difference between firms' raw returns and the returns on their corresponding size-matched decile portfolios. The 1995 CRSP Indices files report size decile portfolio returns for NASDAQ firms starting in 1982.

[4] Collins and Hribar (2000b) suggest that total accruals measured directly from the statement of cash flows (post 1987) are accurate,. while total accruals estimated using a balance sheet approach (prior to 1988) contain measurement error when non-articulation events such as mergers, acquisitions, and divestitures are present. Untabulated analyses reveal that the results reported in my study hold for both pre-1988 and post-1987 periods.

$$ACCR_t/TA_{t-1} = a_1[1/TA_{t-1}] + a_2[\Delta REV_t/TA_{t-1}] + a_3[PPE_t/TA_{t-1}] + e_t, \qquad (1)$$

where ΔREV_t is the change in sales revenues in year t (Compustat item #12) and PPE_t is gross property, plant, and equipment in year t (Compustat item #7). Following DeFond and Jiambalvo (1994) and Subramanyam (1996), I estimate the Jones model in cross-section for each two-digit SIC code and year combination, formed separately for NYSE/AMEX firms and for NASDAQ firms. I denote the predicted values of the Jones model as normal accruals (NAC_t) and the residuals as abnormal accruals ($ABNAC_t$).

I use the 1995 CRSP monthly returns file to measure annual buy-and-hold returns ($RETURN_t$) for the 12-month period ending three months after the firm's fiscal year end. If a stock is delisted during a particular year, I assume the liquidating proceeds are reinvested in the market portfolio for the remainder of the year. Following Sloan (1996), I calculate size-adjusted abnormal returns ($SIZEAJR_t$) as the difference between a firm's annual buy-and-hold return and the annual buy-and-hold return for the same 12-month period on the market-capitalization-based portfolio decile (i.e., size decile) to which the firm belongs. I use the CRSP size decile breakpoints to classify each firm into a size decile according to its market value of equity at the beginning of the calendar year in which the 12-month return period begins.

Panel A of Table 1 provides descriptive statistics for the sample. The results are comparable to those reported in Subramanyam (1996, Table 2), except that the means (medians) of earnings and raw returns are somewhat smaller in my sample.[5] As expected, average total accruals are negative (−0.044) due to depreciation expenses, and average abnormal accruals are near zero (−0.004).[6]

Panel B of Table 1 reports the mean and median firm-specific Pearson and Spearman correlations between selected variables. The Pearson results are comparable to those reported in Subramanyam (1996, Table 5, Panel A), except that the correlations between total accruals and abnormal accruals, and between total accruals and normal accruals are somewhat larger in my sample. Although the correlation between total accruals and abnormal accruals is high (Pearson = 0.80; Spearman = 0.75), abnormal accruals are not the major component of total accruals. The mean (median) ratio of abnormal accruals to total accruals in my sample is 0.37 (0.46) after deleting 168 observations where total accruals are virtually zero (untabulated). Thus, abnormal accruals are a relatively smaller but more variable component of total accruals, whereas normal accruals are a larger but relatively more stable component.

III. TESTS OF THE PRICING OF ABNORMAL ACCRUALS

The Mishkin Test

Mishkin (1983) develops a framework to test the rational expectations hypothesis in macroeconomics (hereafter the Mishkin test). I employ the Mishkin approach to examine whether the market rationally prices abnormal accruals with respect to their one-year-ahead earnings implications. Specifically, I estimate the following regression system:[7]

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[5] Two factors may contribute to this difference. First, Subramanyam (1996) appears to require earnings and other accounting data in the three years after the sample year, while I do not. Imposing this requirement on my sample yields mean (median) earnings of 0.047 (0.054) and mean (median) raw returns of 0.165 (0.079), which are closer to Subramanyam's 0.053 (0.057) for earnings and 0.183 (0.103) for raw returns. Second, Subramanyam's sample does not include firms on the research file. Despite of these differences, I successfully replicate Subramanyam (1996, Tables 3, 6, 7, and 8) on my sample.

[6] Since the Jones model does not have an intercept, the sum of the residuals is not necessarily equal to zero.

[7] I gratefully thank Richard Sloan for his generous help with the Mishkin test method.

In the first stage, I jointly estimate equations (2) and (3) without imposing any constraints on γ^*s and γs. To test whether the valuation coefficients (γ^*s) are significantly different from their counterpart forecasting coefficients (γs) obtained in the first stage, I estimate equations (2) and (3) jointly in the second stage after imposing the rational pricing constraints, $\gamma^*_q = \gamma_q$ (q = 1, 2, and/or 3). Mishkin shows that the following likelihood ratio statistic is asymptotically $\chi^2(q)$ distributed under the null hypothesis that the market rationally prices one or more earnings components with respect to their associations with one-year-ahead earnings:

$$2N\mathit{Ln}(SSR^c/SSR^u),$$

where:

q = the number of rational pricing constraints imposed;
N = the number of sample observations;
Ln = natural logarithm operator;
SSR^c = the sum of squared residuals from the constrained regressions in the second stage; and
SSR^u = the sum of squared residuals from the unconstrained regressions in the first stage.

I reject the rational pricing of one or more earnings components (i.e., $\gamma^*_q = \gamma_q$, q = 1, 2, and/or 3) if the above likelihood ratio statistic is sufficiently large.

Panel A of Table 2 reports the coefficient estimates for equations (2) and (3) obtained in the first stage.[8] For cash from operations, the valuation coefficient ($\gamma^*_1 = 0.67$) is smaller than the forecasting coefficient ($\gamma_1 = 0.73$), suggesting that the market underprices cash from operations relative to its ability to forecast one-year-ahead earnings. To test whether this underpricing is statistically significant, I jointly estimate equations (2) and (3) again in the second stage, after imposing the rational pricing constraint (i.e., $\gamma^*_1 = \gamma_1$). The likelihood ratio statistic of 23.15 reported in Panel B of Table 2 is significant at the 0.0001 level, indicating that the underpricing of cash from operations ($\gamma^*_1 < \gamma_1$) is statistically significant.

Panel A of Table 2 shows that the valuation coefficients the market assigns to normal accruals (γ^*_2) and abnormal accruals (γ^*_3) are 0.78 and 0.69, respectively. These coefficients are larger than their forecasting counterparts ($\gamma_2 = 0.70$, $\gamma_3 = 0.57$). Specifically, γ^*_2 is approximately 11 percent greater than γ_2, and γ^*_3 is 21% greater than γ_3. Panel B of Table 2 reports that the likelihood ratio statistics reject the null hypotheses of rational pricing of normal accruals ($p < 0.001$) and abnormal accruals ($p < 0.0001$). Therefore, the market significantly overprices both normal accruals ($\gamma^*_2 > \gamma_2$) and abnormal accruals ($\gamma^*_3 > \gamma_3$). The overpricing appears more severe for abnormal accruals because the likelihood ratio statistic (277.28) rejects a special case of the null hypothesis that the market overprices normal and abnormal accruals to the same extent ($\gamma^*_2 = \gamma^*_3$ and $\gamma_2 = \gamma_3$). Finally, the likelihood ratio statistic of 147.78 rejects the null hypothesis that the market rationally prices all three earnings components ($p < 0.0001$).

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[8] Coefficient estimates for α, β, and γ_0 are not reported because they have no bearing on the market pricing of earnings components.

When equation (2) is estimated alone using the ordinary least squares method, the forecasting coefficients (γ_1, γ_2, and γ_3) measure the persistence of earnings components (CFO_t, NAC_t, and $ABNAC_t$) with respect to one-year-ahead earnings (see Freeman et al. 1982; Sloan 1996). The forecasting coefficients for CFO_t, NAC_t, and $ABNAC_t$ are 0.73, 0.70, and 0.57, respectively (Panel A, Table 2).[9] Untabulated F-tests conducted on equation (2) alone indicate that the coefficient on cash from operations is significantly larger than that on normal accruals ($F = 29.47$), which, in turn, is significantly larger than the coefficient on abnormal accruals ($F = 267.59$). Therefore, the cash flow component of earnings is more persistent than the normal accrual component, and the normal accrual component is more persistent than the abnormal accrual component.

In summary, the Mishkin test results indicate that abnormal accruals are the least persistent, whereas cash from operations is the most persistent, of the three earnings components. This suggests that the lack of persistence of total accruals Sloan (1996) documents is due primarily to the lack of persistence of abnormal accruals. Since the Mishkin test is a statistical comparison between the market's assessment of the persistence of earnings components (as reflected in its valuation of earnings components by γ^*_1, γ^*_2, and γ^*_3) and the historical persistence of earnings components (as reflected in their association with one-year-ahead earnings by γ_1, γ_2, and γ_3), the Mishkin test results further suggest that the market underestimates the persistence of, and thus underprices, cash from operations. In contrast, the market overestimates the persistence of, and thus overprices, both normal and abnormal accruals, although the market appears to overprice abnormal accruals to a greater extent than it does normal accruals.

The Hedge-Portfolio Test

The Mishkin test suggests that the market acts as if it assigns a *larger* valuation coefficient to abnormal accruals relative to their forecasting coefficient. Consequently, the stock prices of firms with *negative* abnormal accruals will be *lower* than their intrinsic values (i.e., undervalued). On the other hand, the stock prices of firms with *positive* abnormal accruals will be *higher* than their intrinsic values (i.e., overvalued). If a trading strategy that is long in the most negative abnormal accrual decile (i.e., the most undervalued stocks) and short in the most positive abnormal accrual decile (i.e., the most overvalued stocks) yields *positive* abnormal returns in subsequent years, then this would further support inferences from the Mishkin test that the market *over*prices abnormal accruals in the portfolio formation year.

I group firms into portfolio deciles each year based on their ranking of abnormal accruals, and form a hedge portfolio that is long in the most negative abnormal accrual decile and short in the most positive abnormal accrual decile. Panel A of Table 3 reports the average of the 22 annual abnormal size-adjusted returns for each abnormal accrual decile over the 1971–1992 sample period, as well as the abnormal returns to the hedge portfolio. The numbers in parentheses are t-statistics based on the mean and standard errors of the 22-year time-series. The size-adjusted abnormal returns for the most negative abnormal accrual decile are significantly positive in years $t + 1$ (0.049, $t = 2.82$), $t + 2$ (0.041, $t = 2.32$), and $t + 3$ (0.024, $t = 2.05$). In contrast, the size-adjusted abnormal returns for the most positive abnormal accrual decile are significantly negative in years

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[9] Forecasting coefficients obtained from the first stage of the Mishkin test (when no rational pricing constraints are imposed) are identical to those obtained from estimating equation (2) alone using the ordinary least squares method. Subramanyam (1996, Table 7) estimates equation (2) alone and reports similar results.

TABLE 3
Time-series Means (t-statistics) of Annual Size-Adjusted Abnormal Returns for Each Portfolio in Three Years after Portfolio Formation

Portfolio	*Panel A: Abnormal Accruals*			*Panel B: Normal Accruals*		
Ranking[a]	*Year t + 1*	*Year t + 2*	*Year t + 3*	*Year t + 1*	*Year t + 2*	*Year t + 3*
Lowest (−)	0.049	0.041	0.024	0.015	0.008	0.011
	(2.82)**	(2.32)*	(2.05)*	(0.68)	(0.43)	(0.54)
2	0.055	0.040	0.027	0.022	0.034	0.025
	(5.32)**	(3.14)**	(2.87)**	(1.79)	(2.71)**	(2.43)*
3	0.036	0.034	0.027	0.004	0.016	0.038
	(3.57)**	(3.81)**	(3.23)**	(0.39)	(1.18)	(4.00)**
4	0.028	0.015	0.035	0.023	0.023	0.004
	(3.45)**	(1.78)	(4.00)**	(1.61)	(2.03)	(0.28)
5	0.032	0.014	0.017	0.032	0.020	0.016
	(4.02)**	(1.65)	(2.12)*	(2.71)**	(1.79)	(1.67)
6	0.027	0.028	0.014	0.031	0.024	0.028
	(2.85)**	(2.71)**	(1.64)	(2.97)**	(3.09)**	(4.74)**
7	−0.002	0.006	0.005	0.022	0.024	0.012
	(−0.30)	(0.51)	(0.53)	(2.41)*	(2.13)*	(1.22)
8	0.005	0.022	0.008	0.010	0.011	0.019
	(0.87)	(2.83)**	(0.82)	(0.76)	(1.48)	(1.83)
9	−0.011	0.004	0.009	0.004	0.010	0.007
	(−1.46)	(0.45)	(0.78)	(0.46)	(0.96)	(0.83)
Highest (+)	−0.061	−0.033	0.005	−0.007	−0.003	0.009
	(−4.86)**	(−3.05)**	(0.36)	(−0.37)	(−0.19)	(0.63)
Hedge	0.110	0.074	0.019	0.023	0.011	0.002
	(8.43)**	(5.78)**	(1.58)	(0.78)	(0.53)	(0.07)
n	51,579	48,236	44,499	51,579	48,236	44,499

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* and ** denote significance at the 0.05 and 0.01 level, respectively, based on a two-tailed t-test for the time-series (22 years) of annual portfolio abnormal returns.

[a] Portfolio deciles are formed annually based on the ranking of abnormal and normal accruals for Panel A and Panel B, respectively. The hedge portfolio is formed by taking a long position in the lowest decile portfolio and a short position in the highest decile portfolio based on abnormal and normal accruals, respectively.

The variables are defined in Table 1.

t + 1 (−0.061, t = −4.86) and t + 2 (−0.033, t = −3.05), and insignificantly different from zero in year t + 3 (0.005, t = 0.36). Thus, the hedge portfolio yields positive size-adjusted abnormal returns of 11.0 percent (t = 8.43), 7.4 percent (t = 5.78), and 1.9 percent (t = 1.58) in years t + 1, t + 2, and t + 3, respectively. The significantly positive abnormal returns to the hedge portfolio in years t + 1 and t + 2 are consistent with the market overpricing abnormal accruals in the portfolio formation year (year t).

Untabulated results reveal that the size-adjusted abnormal returns to the hedge portfolio in year t + 1 are positive for each of the 22 sample years. This suggests that unidentified risk factors are unlikely to explain the abnormal returns. Rather, the year-after-year positive abnormal returns to the hedge portfolio are more consistent with the market overpricing abnormal accruals.

When I form the hedge portfolio based on normal accruals, the average annual abnormal returns in years t + 1, t + 2, and t + 3 for both the most negative and the most positive normal accrual deciles are all insignificantly different from zero. The average annual abnormal returns to the hedge portfolio are 2.3 percent (t = 0.78), 1.1 percent (t = 0.53), and 0.2 percent (t = 0.07) in years t + 1, t + 2, and t + 3, respectively (see Panel B, Table 3). The fact that abnormal returns to the normal accrual-based hedge portfolio are not significantly different from zero suggests that the market does not misprice normal accruals in year t. Moreover, untabulated results reveal that the normal accrual-based hedge portfolio does not yield consistently positive abnormal returns, as the year t + 1 abnormal returns are negative in seven of the 22 sample years. Overall, the hedge-portfolio test results do not suggest that the market overprices normal accruals, which is inconsistent with the inferences from the Mishkin test.

This inconsistency is likely due to the fact that I conducted the Mishkin test on the entire (larger) sample, whereas I conducted the hedge-portfolio test on the most positive and most negative deciles (20 percent of the sample). When I use only December fiscal year-end firms (29,967 observations, 52.86 percent of the sample) and conduct both Mishkin and hedge-portfolio tests on the most positive and most negative normal accrual deciles of this reduced sample, neither the Mishkin test nor the hedge-portfolio test detects any mispricing of normal accruals. Similarly, when I randomly reduce the entire sample by half (randomly choosing half of the firms in each sample year) and conduct the Mishkin and hedge-portfolio tests on the most positive and most negative normal accrual deciles of the reduced sample, neither test detects any mispricing of normal accruals.

My sample includes both December fiscal year-end and non-December fiscal year-end firms. Accounting information for a given fiscal year will become available to the market at different points in calendar time for firms with different fiscal year-end months. Thus, one cannot directly implement the hedge-portfolio strategies reported in Table 3. To address this concern, I form 12 hedge portfolios, one for each fiscal year-end month. I then conduct the hedge-portfolio test separately for each of the 12 fiscal year-end months. The results are qualitatively identical to those reported in Table 3. For example, the abnormal accrual-based hedge portfolio for December fiscal year-end firms yields abnormal returns of 10.10 percent (t = 6.01), 4.84 percent (t = 3.29), and 3.67 percent (t = 2.36) in years t + 1, t + 2, and t + 3, respectively. On the other hand, the abnormal returns to the normal accrual-based hedge portfolio are 1.07 percent (t = 0.36), 1.93 percent (t = 0.72), and 0.23 percent (t = 0.07) in years t + 1, t + 2, and t + 3, respectively. The abnormal returns for the 11 non-December fiscal year-end hedge portfolios are qualitatively similar. Thus, one can think of the abnormal returns reported in Table 3 as the *average* abnormal returns from these 12 implementable hedge portfolios with the same fiscal year-end months.

Finally, I repeat the hedge-portfolio test using the Fama and French (1993) three-factor model to estimate abnormal returns to the hedge portfolios for December fiscal year-end firms. For the abnormal accrual-based hedge portfolio, the abnormal returns in years t + 1, t + 2, and t + 3 are 9.60 percent (t = 4.59), 4.32 percent (t = 2.51), and 3.00 percent (t = 1.65), respectively.[10] These returns are very similar to the size-adjusted abnormal returns reported above for December fiscal year-end firms. The abnormal returns to the normal accrual-based hedge portfolio are 2.28 percent (t = 0.92), 4.32 percent (t = 1.71), and 1.92 percent (t = 0.78) in years t + 1, t + 2, and t + 3, respectively, which are again qualitatively similar to the size-adjusted abnormal returns reported above.

To summarize, the hedge-portfolio results corroborate the Mishkin test finding that the market overprices abnormal accruals, but they do not support the overpricing of normal accruals detected by the Mishkin test. On the whole, I conclude that the market overprices abnormal accruals but does not materially misprice normal accruals, and that Sloan's (1996) finding that the market overprices total accruals is due largely to abnormal accruals.

IV. SENSITIVITY ANALYSIS

Alternative Abnormal Accrual Models

The results reported in this paper are based on the cross-sectional Jones model. I examine the results' robustness to five alternative abnormal accrual models: (1) the cross-sectional modified Jones model (Dechow et al. 1995); (2) the time-series Jones model; (3) the time-series modified Jones model (Dechow et al. 1995); (4) the Beneish (1997) model, which expands the Jones model by adding lagged total accruals and lagged stock returns as two additional explanatory variables; and (5) the Beneish (1998) model, which replaces "change in sales" in the Jones model by "change in cash sales." These five models produce abnormal and normal accrual measures that are highly correlated with the cross-sectional Jones model-estimated abnormal and normal accruals, suggesting considerable overlap between abnormal accruals estimated by the cross-sectional Jones model and by the five alternative models.[11]

I repeat the Mishkin test and the hedge-portfolio test using the five alternative models. The Mishkin test suggests that (1) the market overprices abnormal accruals based on each of the five models; (2) the market also overprices normal accruals (but to a lesser extent than abnormal accruals), except for the Beneish (1998) model, where normal accruals appear to be rationally priced; and (3) the market underprices cash from operations, except for the time-series Jones model and the time-series modified Jones model, where cash from operations appears to be rationally priced. The hedge-portfolio test results suggest that

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[10] Specificially, I estimate the following regression for December fiscal year-end firms:

$$R_{p\tau} - R_{f\tau} = \alpha_0 + \beta_1(RM_\tau - R_{f\tau}) + \beta_2 SMB_\tau + \beta_3 HML_\tau + \varepsilon_{p\tau} \quad (5F)$$

where $R_{p\tau}$ = stock return of portfolio p in month τ; $R_{f\tau}$ = risk-free rate in month τ; RM_τ = market return in month τ; SMB_τ = size factor (small minus big) in month τ; and HML_τ = book-to-market factor (high minus low) in month τ. I rank firms annually into portfolio deciles based on abnormal accruals. In each sample year t (from 1971 to 1992), I calculate 12 equally weighted monthly portfolio returns in year t + 1 (i.e., $R_{p\tau}$), starting from April, year t + 1, to March, year t + 2, for the most positive and the most negative abnormal accrual deciles. Thus, I obtain a time-series of 264 (22 years × 12 months) observations of monthly portfolio returns for each decile. I then run equation (5F) using these 264 monthly portfolio returns for each decile. The intercept α_0 measures the average monthly abnormal return of that decile in year t + 1. I multiply α_0 by 12 to get the annualized abnormal return. The abnormal return to the abnormal accrual-based hedge portfolio in year t + 1 is the difference between the annualized abnormal return of the most negative abnormal accrual decile (long position) and that of the most positive abnormal accrual decile (short position). I estimate the abnormal returns to the hedge portfolio in year t + 2 and t + 3 similarly.

[11] The pairwise Pearson correlations between abnormal accruals used in the paper and those estimated by the five alternative models range from 0.73 (with the time-series modified Jones model) to 0.98 (with the cross-sectional modified Jones model). Similarly, the pairwise Pearson correlations between normal accruals used in the paper and those estimated by the five alternative models range from 0.57 (with the time-series modified Jones model) to 0.96 (with the cross-sectional modified Jones model).

(1) the market overprices abnormal accruals based on each of the five models, and (2) the market approximately rationally prices normal accruals, except for the time-series Jones model, where it slightly overprices normal accruals (size-adjusted abnormal return in year $t + 1 = 0.043$, $t = 2.55$). Thus, the paper's main conclusion, that the market overprices abnormal accruals relative to their one-year-ahead earnings implications, is robust to all five alternative abnormal accrual models.

Controls for Major Unusual Accruals

The Jones model-estimated abnormal accruals capture both managerial discretion and unusual nondiscretionary accruals (Healy 1996; Bernard and Skinner 1996). Consequently, it is difficult to determine whether the market overprices the portion of abnormal accruals stemming from discretionary managerial behavior or the portion arising from unusual business circumstances. Following Bernard and Skinner (1996) and Collins and Hribar (2000b), I estimate abnormal accruals after controlling for major unusual accruals and non-articulation events. I then test whether the market still misprices this refined measure of abnormal accruals.

Bernard and Skinner (1996, 317) suggest that special items such as nonoperating gains and losses are often not discretionary. However, the Jones model misclassifies them as "discretionary" (i.e., as unexplained residuals) because they are typically not linearly related to changes in revenues. Since I define total accruals as the difference between earnings before extraordinary items and cash from operations, total accruals include the effect of special items. As a first step in controlling for major unusual but largely nondiscretionary accruals, I remove after-tax special items from earnings, and thus from total accruals. Specifically, I define this refined measure of total accruals ($ACCR2_t$) as follows: $ACCR2_t = EARN_t - 0.6 \times SPECIAL_t - CFO_t$, where $SPECIAL_t$ = special items (Compustat item #17).[12] I define other variables as before.

Collins and Hribar (2000b) demonstrate that when the presumed articulation between the balance sheet and the income statement breaks down (due to non-articulation events such as mergers, acquisitions, and divestitures), total accruals estimated using a balance-sheet approach contain material measurement error, relative to total accruals *correctly* measured from the statement of cash flows. They further demonstrate that this measurement error in the balance-sheet-estimated total accruals flows directly through to the Jones model residuals, contaminating these residuals' ability to capture managerial discretion when mergers, acquisitions, and divestitures are present. I estimate total accruals using a balance-sheet approach for the 1971–1987 period because cash from operations is unavailable until 1988. Thus, the measurement error Collins and Hribar identify directly applies to that period of my sample. In addition, mergers, acquisitions, and divestitures are important "unusual business circumstances" that may lead to unusual but nondiscretionary accruals. To the extent that these unusual accruals are not linearly related to changes in revenues, the Jones model will misclassify them as "discretionary" even when total accruals are correctly measured from the statement of cash flows, as in my 1988–1992 period.

Thus, in the spirit of Collins and Hribar (2000b), to better proxy for managerial discretion using the Jones model, I delete observations with mergers and acquisitions (Compustat footnote #1) or divestitures (defined as the absolute value of discontinued operations, Compustat item #66, exceeding $10,000) in order to control for (1) measurement error in the balance-sheet-estimated total accruals due to non-articulation events and (2) unusual but largely nondiscretionary accruals arising from these unusual business circumstances. I then use my refined measure of total accruals ($ACCR2_t$) and the Jones model (equation [1]) to estimate abnormal accruals for the subsample of firm-years without mergers, acquisitions, or divestitures. As before, I estimate the Jones model in cross-section for each two-digit SIC code and year combination. I denote the predicted values as normal accruals ($NAC2_t$) and the residuals as abnormal accruals ($ABNAC2_t$).

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[12] I assume that the marginal tax rate is 40 percent for all my sample firms and years. Changing the marginal tax rate to 30 percent does not qualitatively change the results.

Table 4 reports the Mishkin test results using the refined measures of normal and abnormal accruals. The forecasting coefficient for each earnings component is larger than its counterpart in Table 2, likely because the earnings stream becomes more persistent after I purge after-tax special items from earnings. The Mishkin test indicates that the market underprices cash from operations ($\gamma^*_1 = 0.75$, $\gamma_1 = 0.80$, $p < 0.001$), but that it does not misprice normal accruals ($\gamma^*_2 = 0.78$, $\gamma_2 = 0.75$, $p < 0.35$). More importantly, the market overprices abnormal accruals ($\gamma^*_3 = 0.80$, $\gamma_3 = 0.66$, $p < 0.0001$). Finally, untabulated analyses indicate that the size-adjusted abnormal returns to the abnormal accrual-based hedge portfolio are 11.54 percent ($t = 5.35$), 4.19 percent ($t = 2.37$), and 2.81 percent ($t = 1.78$) in years $t + 1$, $t + 2$, and $t + 3$, respectively. In contrast, the abnormal returns to the normal accrual-based hedge portfolio are 1.85 percent ($t = 0.61$), 0.14 percent ($t = 0.05$), and −0.46 percent ($t = -0.18$) in years $t + 1$, $t + 2$, and $t + 3$, respectively. These results suggest that the market overprices abnormal accruals, but that it does not misprice normal accruals, consistent with the Mishkin test results. Thus, both the Mishkin test and the hedge-portfolio test indicate that the market still overprices the refined measure of abnormal accruals. Since this measure is more likely to capture accruals arising from managerial discretion, the above finding is consistent with the market overpricing the portion of abnormal accruals stemming from earnings management.

V. CONCLUSION

This paper examines whether the market rationally prices Jones model-estimated abnormal accruals. The Mishkin test results suggest that the market overestimates the persistence of, and thus overprices, both abnormal and normal accruals, although the overpricing of abnormal accruals is more severe. The hedge-portfolio test results corroborate the overpricing of abnormal accruals, but do not support the overpricing of normal accruals. Taken together, my results suggest that the market overprices abnormal accruals, while the evidence on the overpricing of normal accruals is mixed and weak.

This paper extends Subramanyam's (1996) finding that the market prices abnormal accruals, by providing direct evidence that the market overprices abnormal accruals relative to their association with one-year-ahead earnings. The study also extends Sloan (1996) by suggesting that the lack of persistence and the overpricing of total accruals he reports are due largely to abnormal accruals. Finally, this paper extends Teoh et al. (1998a, 1998b) and Rangan (1998) by suggesting that the overpricing of abnormal accruals arises in general contexts and is not limited to IPOs or seasoned equity offerings.

Following suggestions in Bernard and Skinner (1996) and Collins and Hribar (2000b), I control for major unusual accruals and non-articulation events when estimating abnormal accruals using the Jones model. I find that the market still overprices this refined measure of abnormal accruals. This is consistent with the notion that the market overprices the portion of abnormal accruals stemming from managerial discretion.

This study raises several issues for future research. First, Jones model-estimated abnormal accruals capture managerial discretion with error. Although my sensitivity analysis controls for major unusual accruals and non-articulation events, it is only a first step in developing a better proxy for managerial discretion. Future research could investigate other

TABLE 4

Nonlinear Generalized Least Squares Estimation (the Mishkin Test) of the Market Pricing of Cash from Operations, Normal Accruals, and Abnormal Accruals with Respect to Their Implications for One-Year-Ahead Earnings—with Controls for Major Unusual Accruals

Panel A: Market Pricing of Earnings Components with Respect to Their Implications for One-year-ahead Earnings

$$EARN2_{t+1} = \gamma_0 + \gamma_1 CFO_t + \gamma_2 NAC2_t + \gamma_3 ABNAC2_t + \upsilon_{t+1} \quad (2)$$

$$SIZEAJR_{t+1} = \alpha + \beta(EARN2_{t+1} - \gamma_0 - \gamma^*_1 CFO_t - \gamma^*_2 NAC2_t - \gamma^*_3 ABNAC2_t) + \varepsilon_{t+1} \quad (3)^{a,b}$$

Forecasting Coefficients			*Valuation Coefficients*		
Parameter	*Estimate*	*Asymptotic Std. Error*	*Parameter*	*Estimate*	*Asymptotic Std. Error*
γ_1 (CFO)	0.80	0.004	γ^*_1 (CFO)	0.75	0.013
γ_2 (NAC2)	0.75	0.008	γ^*_2 (NAC2)	0.78	0.026
γ_3 (ABNAC2)	0.66	0.006	γ^*_3 (ABNAC2)	0.80	0.020

Panel B: Test of Rational Pricing of Earnings Components

Null Hypotheses	*Likelihood Ratio Statistic*	*Marginal Significance Level*
CFO: $\gamma^*_1 = \gamma_1$	11.55	<0.001
NAC2: $\gamma^*_2 = \gamma_2$	1.08	<0.35
ABNAC2: $\gamma^*_3 = \gamma_3$	42.57	<0.0001
NAC2, ABNAC2: $\gamma^*_2 = \gamma^*_3$ and $\gamma_2 = \gamma_3$	91.61	<0.0001
CFO, NAC2, ABNAC2: $\gamma^*_1 = \gamma_1$ and $\gamma^*_2 = \gamma_2$ and $\gamma^*_3 = \gamma_3$	99.53	<0.0001

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[a] Equations (2) and (3) are jointly estimated using an iterative generalized nonlinear least squares estimation procedure based on the subsample of 43,408 observations during 1971–1992. The subsample deletes observations where mergers and acquisitions (Compustat footnote #1) are present or divestitures, defined as the absolute value of discontinued operations (Compustat item #66) exceeding $10,000, are present. Due to missing values for the dependent variables, the number of observations used is 33,262.

[b] Variable definitions:

$EARN2_t$ = income before extraordinary items (Compustat item #18) − 0.6 × $SPECIAL_t$;

$SPECIAL_t$ = special items (Compustat item #17);

$ACCR2_t$ = a refined measure of total accruals = $EARN2_t - CFO_t$;

$NAC2_t$ = a refined measure of normal accruals = predicted values of the Jones (1991) model estimated in cross-section for each two-digit SIC code and year combination when the dependent variable is $ACCR2_t$; and

$ABNAC2_t$ = a refined measure of abnormal accruals = residual values of the Jones (1991) model estimated in cross-section for each two-digit SIC code and year combination when the dependent variable is $ACCR2_t$.

All variables except $SIZEAJR_t$ are deflated by beginning-of-year total assets (Compustat item #6). Other variables are defined in Table 1.

factors that systematically contaminate the Jones (1991) model residuals' ability to capture managerial discretion or develop a better discretionary accrual model. Second, this paper examines whether the market misprices abnormal accruals relative to their implications for one-year-ahead earnings. This is only one dimension of market mispricing. Future research could investigate other dimensions or forms of market mispricing. Finally, although I find that the abnormal returns to the abnormal accrual-based hedge portfolio in year t + 1 are positive in each of my 22 sample-years, it is difficult to completely rule out unknown risk factors as an alternative explanation for the results.

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The Reversal of Abnormal Accruals and the Market Valuation of Earnings Surprises

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ABSTRACT: If the market anticipates the reversing nature of *abnormal* working capital accruals, then the *reported* magnitude of earnings surprises that contain abnormal accruals will differ from the *underlying* magnitude that is priced by the market. We expect the market's perception of this difference to affect the ERCs associated with earnings surprises that contain abnormal accruals. We test our predictions using an abnormal accruals measure that captures the difference between reported working capital and a proxy for the market's expectations of the level of working capital required to support current sales levels. Consistent with our hypotheses, we find higher ERCs when abnormal accruals suppress the magnitude of earnings surprises, and lower ERCs when abnormal accruals exaggerate the magnitude of earnings surprises. We also find results consistent with analysts predictably considering the reversing implications of abnormal accruals in revising future earnings forecasts. These findings are consistent with market participants anticipating the reversing implications of abnormal accruals. However, analysis of subsequent stock returns provides evidence that market participants do not fully impound the pricing implications of abnormal accruals at the earnings announcement date.

Keywords: *accruals; abnormal accruals; earnings surprise; earnings response coefficient.*

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Data Availability: *The data used in this study are publicly available from the sources listed in the study.*

I. INTRODUCTION

The role of accounting earnings in pricing securities is a fundamentally important issue in accounting research (Ball and Brown 1968; Watts and Zimmerman 1986; Beaver 1998). Prior studies find that accounting accruals play an important role in helping market participants value securities (Bowen et al. 1987; Dechow 1994; Subramanyam 1996). However, research also suggests that market participants may not fully understand the information conveyed by accruals (Sloan 1996; Xie 2001). This study further investigates the role of accruals in pricing securities by testing whether the market's pricing of earnings surprises anticipates the reversing implications of abnormal accruals.

We argue that the reversing nature of *abnormal* working capital accruals suggests that they have little or no net effect on lifetime earnings, and therefore should be priced differentially by the market. For example, understating the allowance for bad debts results in abnormal accruals that increase current earnings. When the firm eventually recognizes the bad debt expenses, the abnormal accruals reverse, thereby decreasing future earnings. Thus, the income-increasing effect of the abnormal accruals is later offset by the income-decreasing effect of the reversal. Abnormal accruals that have little or no net effect on lifetime earnings should have little or no stock price impact in reasonably efficient capital markets.[1] Thus, we expect the *reported* magnitude of earnings surprises that contain abnormal accruals to differ from the *underlying* magnitude that the market prices. We test inferences about the market's pricing of abnormal accruals using an abnormal accruals measure that captures the difference between reported working capital and a proxy for the market's expectations of the level of working capital required to support current sales levels.

We expect the market's perception of the magnitude of earnings surprises to depend upon both the effect of the abnormal accruals on income and the sign of the earnings surprise. Income-*increasing* abnormal accruals exaggerate the magnitude of *good* news earnings surprises and, therefore, we expect the market to infer that the underlying surprise is actually smaller than reported.[2] If the market perceives that the underlying good news surprise is smaller than the reported surprise, we expect a lower earnings response coefficient (ERC) associated with the reported surprise. On the other hand, income-*decreasing* abnormal accruals suppress the magnitude of *good* news earnings surprises, so we expect the market to infer that the underlying surprise is larger than reported. In this case, we expect a higher ERC associated with the reported surprise. Therefore, our first hypothesis is that good news firms with income-increasing abnormal working capital accruals have lower ERCs than good news firms with income-decreasing abnormal working capital accruals.

Abnormal accruals have the opposite implications for reported bad news surprises. Income-increasing abnormal accruals suppress the magnitude of bad news earnings surprises and thus should lead the market to infer that the underlying surprise is actually larger than reported. Conversely, income-decreasing abnormal accruals exaggerate the magnitude of bad news surprises, suggesting that the underlying surprise is smaller than reported.

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[1] Because earnings shift across periods, differences in factors such as marginal tax rates and bonus thresholds mean that the reversal may not precisely offset the initial abnormal accrual. However, we expect the majority of the abnormal accruals to reverse against future earnings.

[2] We refer to firms reporting positive (negative) earnings surprises relative to analysts' forecasts as "good (bad) news firms."

Thus, our second hypothesis is that bad news firms with income-increasing abnormal working capital accruals have higher ERCs than bad news firms with income-decreasing abnormal working capital accruals.

Our sample consists of 14,839 firm-quarter earnings announcements from the First Call Historical Database (hereafter First Call) from 1992 through 1995. Conceptually, our abnormal accruals measure captures the difference between *realized* working capital and a proxy for the market's *expectations* of the level of working capital required to support current sales levels. Intuitively, this difference represents working capital accruals that are unlikely to be sustained in the future and, therefore, are likely to reverse against future earnings.[3] Consistent with our predictions, for good news firms the ERC is significantly higher for firms reporting income-decreasing abnormal accruals (6.1) than for firms reporting income-increasing abnormal accruals (4.6). By contrast, for bad news firms the ERC is significantly higher for firms reporting income-increasing abnormal accruals (1.1) than for firms reporting income-decreasing abnormal accruals (0.6). These results are robust after controlling for number of analysts, firm size, the market-to-book ratio, growth in book value of equity, earnings volatility, and earnings persistence.

We also examine whether analysts' revisions of their forecasts of next quarter's earnings, after this quarter's earnings announcement, anticipate the reversing implications of abnormal accruals. We find that analysts make smaller upward forecast revisions in response to *good* news surprises that contain income-increasing abnormal accruals, and larger downward forecast revisions in response to *bad* news surprises that contain income-increasing abnormal accruals. This is consistent with analysts anticipating the reversing implications of abnormal accruals, and corroborates the findings in our primary hypotheses tests.

While these results are consistent with the market anticipating the reversing implications of abnormal accruals, they do not address whether the market appropriately prices the earnings surprises in anticipation of the reversing behavior of the abnormal accruals. Therefore, we also examine cumulative abnormal returns associated with the accruals partitions during the 80 trading days following the earnings announcement. We find significant post-announcement abnormal returns associated with the accruals partitions and estimate that the market only anticipates 19–23 percent of the pricing implications of the abnormal accruals.

Our evidence that the market anticipates the reversing implications of abnormal accruals, but only to a limited degree, is consistent with prior evidence that the market does not fully adjust for other implications of accrual-based earnings.[4] To the extent that abnormal accruals reflect intentional misstatements rather than unusual nondiscretionary events, our findings are consistent with market participants at least partially adjusting for suspected earnings management. Our results are also consistent with inferences drawn in contemporaneous, independently developed research reported in Xie (2001). Xie concludes that the market misprices abnormal accruals by overestimating the persistence of abnormal accruals. However, research design differences hinder direct comparisons of the two studies.[5]

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[3] We use the term "abnormal" to characterize our accruals estimates while similar estimates from the Jones model are interchangeably referred to as "discretionary" (for example in Jones 1991) or "abnormal" (for example in DeFond and Jiambalvo 1994). Healy (1996) observes that "abnormal" is more accurate because "discretionary" connotes purposeful intervention by management and the Jones model estimates can also include inadvertent misstatements. Therefore, we believe that "abnormal" appropriately characterizes our accruals measure.

[4] For example, evidence suggests that the market does not fully adjust for earnings surprises (Foster et al. 1984; Freeman and Tse 1989; Bernard and Thomas 1990), transitory items (Hand 1990), or the reduced persistence of extreme total accruals (Sloan 1996; Bradshaw et al. 1999).

We also use an abnormal working capital accruals model that differs from the Jones-type models as popularly specified in prior research (Jones 1991; Dechow et al. 1995; Han and Wang 1998). The Jones-type models base their estimates of expected accruals on the coefficient from a regression of accruals on the change in sales, and include an intercept term equivalent to the average level of accruals (after controlling for the change in sales). In contrast, our model estimates expected accruals based upon the firm-specific, seasonally adjusted ratio of working capital to sales.[6]

The next section discusses our hypotheses development, Section III explains the research design and Section IV presents the results. Section V reports several robustness checks and we summarize our findings in Section VI.

II. HYPOTHESES DEVELOPMENT

Working capital accruals represent the change in noncash working capital accounts such as inventories, receivables and accrued expenses. Dechow (1994, Table 2) and Dechow et al. (1998, 141) theoretically and empirically demonstrate that, if sales follow a random walk and normal working capital is a fixed proportion of sales, normal working capital *accruals* follow a mean zero, white noise time-series process. For example, suppose a demand shock permanently increases sales by $100 each period. Assume *normal* working capital equals 20 percent of sales, and for simplicity consists only of accounts receivable. In this simple example, the $100 increase in sales results in a *permanent* $20 increase in accounts receivable. This means that earnings include a $20 income-increasing accrual in the period of the demand shock. During the next period, *ceteris paribus*, the $20 increase in receivables reverses when the account is collected by crediting accounts receivable and debiting cash, but it is replaced with another $20 in receivables. Thus, earnings effectively include a one-time $20 income-increasing accrual in the period of the demand shock.

However, unlike *normal* working capital accruals, *abnormal* working capital accruals eventually reverse (without replacement) against future earnings, so they have a smaller *net* impact on lifetime earnings.[7] For example, understating the allowance for bad debts results in abnormal accruals that increase current period earnings. When the allowance for bad debts is ultimately restored to its correct balance, crediting the allowance account and debiting bad debt expense reverses the abnormal accruals. The net result is that the income-*increasing* effect of the abnormal accrual is ultimately offset by the income-*decreasing* effect of the reversal. In this example, abnormal accruals result from discretionary manipulation, and have *no* net impact on lifetime earnings.

Efficient capital markets should differentially price abnormal accruals that have little or no impact on lifetime earnings. Therefore, we expect the *reported* magnitude of earnings surprises containing abnormal accruals to differ from the *underlying* magnitude that the market prices. For example, a reported good news earnings surprise that contains income-decreasing abnormal accruals effectively understates the magnitude of the true underlying good news. The market's perception of the underlying magnitude of the earnings surprise affects the slope coefficient of the returns/earnings regression, or ERC (Kormendi and Lipe 1987; Collins and Kothari 1989; Easton and Zmjewski 1989). Theoretically, if the earnings surprise is largely permanent, the ERC equals 1 + (1/r) where r is the cost of capital. However, if the *underlying* surprise is perceived to exceed the *reported* surprise, the estimated ERC will be higher. For example, if a *reported* good news surprise is $2 and it contains an income-decreasing abnormal accrual of $1 that will reverse with no net impact on earnings, the *underlying* surprise is actually $3. If the return associated with the underlying good news is $12, then the ERC should be 4 ($12/$3). However, since the reported good news is only $2, the estimated ERC is 6 ($12/$2).

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[5] For example, Xie (2001) uses a long-window setting to examine abnormal accruals as measured by the model in Jones (1991), while our study uses a short-window setting to examine an accruals measure that differs from the Jones model (as discussed in Section V).

[6] This is consistent with Dechow et al. (1998) who model working capital as a fixed proportion of contemporaneous sales.

[7] Abnormal accruals may arise from operational reasons (Bernstein and Wild 1999) or earnings management (Watts and Zimmerman 1986; Healy and Wahlen 1999). Examining a sample of 1,821 companies over 36 quarters from 1988 through 1996 from the Compustat database, we find that abnormal accruals have negative first-order serial-correlation (both mean and median). This is consistent with abnormal working capital accruals reversing against future earnings.

While income-*decreasing* abnormal accruals *understate* the magnitude of reported good news surprises, income-*increasing* abnormal accruals *overstate* the magnitude of good news surprises. If market participants understand the reversing implications of abnormal accruals, then we expect, *ceteris paribus*, higher ERCs associated with good news surprises that contain income-decreasing abnormal accruals, and lower ERCs associated with good news surprises that contain income-increasing abnormal accruals. Thus, our first hypothesis, in alternative form, is:

Hypothesis 1: Good news firms with income-increasing abnormal working capital accruals have lower ERCs than good news firms with income-decreasing abnormal working capital accruals.

Abnormal accruals have the opposite implications for bad news earnings surprises. Income-*decreasing* abnormal accruals *overstate* the magnitude of reported bad news surprises, and income-*increasing* abnormal accruals *understate* the magnitude of bad news surprises. Thus, our second hypothesis, in alternative form, is:

Hypothesis 2: Bad news firms with income-increasing abnormal working capital accruals have higher ERCs than bad news firms with income-decreasing abnormal working capital accruals.

III. RESEARCH DESIGN

Hypotheses Tests

We test our hypotheses by examining the ERCs from a regression of two-day cumulative stock returns on earnings forecast errors after partitioning on the signs of (1) the forecast error and (2) the abnormal working capital accruals. We use market-adjusted returns measured as the difference between the raw stock return and the return on the CRSP value-weighted market portfolio. Our ERCs are estimated from the following model:

$$\begin{aligned} \text{CAR} = \alpha &+ \beta_1(\text{GOODNEWS} \times \text{FE}/\text{P}_{-2}) + \beta_2(\text{GOODNEWSINCR} \times \text{FE}/\text{P}_{-2}) \\ &+ \beta_3(\text{BADNEWS} \times \text{FE}/\text{P}_{-2}) + \beta_4(\text{BADNEWSINCR} \times \text{FE}/\text{P}_{-2}) + \varepsilon \qquad (1) \end{aligned}$$

where:

CAR = market-adjusted stock returns accumulated over the two trading days [−1,0] where 0 is the *Wall Street Journal* earnings announcement date;
FE/P_{-2} = forecast error for the current period earnings announcement, computed as actual earnings for the current quarter minus the most recent analysts' forecast, scaled by the closing share price at trading day −2;
GOODNEWS = good news dummy that equals 1 if actual earnings exceed forecast;
GOODNEWSINCR = good news income-increasing dummy that equals 1 if GOODNEWS equals 1 and abnormal working capital accruals increase income;
BADNEWS = bad news dummy that equals 1 if actual earnings fall short of forecast;
BADNEWSINCR = bad news income-increasing dummy that equals 1 if BADNEWS equals 1 and abnormal working capital accruals increase income; and
ε = error term.

We support our first hypothesis if the coefficient on the good news income-increasing partition (β_2) is negative, and we support our second hypothesis if the coefficient on the bad news income-increasing partition (β_4) is positive.

Abnormal Working Capital Accruals

At a conceptual level, our proxy for abnormal accruals measures the difference between realized working capital, and a proxy for the market's expectations of the level of working capital needed to support current sales levels. The intuition behind our measure is that this difference is the portion of working capital accruals that are unlikely to be sustained and, therefore, are expected to reverse against future earnings. The Appendix develops our empirical measure, which is the difference between the current quarter's realized working capital, and the *expected* level of working capital, where the historical relation of working capital to sales captures expected working capital.[8] Specifically, our empirical abnormal accruals proxy is:[9]

$$AWCA_t = WC_t - [(WC_{t-4}/S_{t-4}) \times S_t] \tag{2}$$

where:

t = year-quarter; thus t − 1 refers to the prior quarter and t − 4 refers to the same quarter in the prior year;
$AWCA_t$ = abnormal working capital accruals in the current quarter;
WC_t = noncash working capital in the current quarter computed as (current assets − cash and short-term investments) − (current liabilities − short-term debt);

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[8] Using the historical relation of working capital to sales to capture expected working capital is consistent with Dechow et al. (1998) who model working capital as a fixed proportion of contemporaneous sales.

[9] As discussed in Section V, our model differs from the Jones-type abnormal accruals models. For example, we estimate abnormal accruals using a firm-specific seasonally adjusted ratio of working capital to sales, while the Jones-type models typically do not allow for seasonality in the relation between accruals and sales changes.

WC_{t-4} = working capital in the same quarter last year;
S_t = sales in the current quarter; and
S_{t-4} = sales in the same quarter last year.

Ideally, our model of abnormal accruals would incorporate analysts' forecasts of accruals, consistent with our model of unexpected earnings. However, analysts' forecasts of accruals (or cash flows) are not widely available and, therefore, our mechanical model-based abnormal accruals estimates are probably less precise than our unexpected earnings estimates. While this is a limitation, we only rely on the model for the sign of the abnormal accrual predictions (i.e., simply distinguishing between income-increasing and income-decreasing abnormal accruals).

In addition, investors must have balance sheet information to compute abnormal accruals. Of the 12,576 earnings announcements in our sample with press releases available on the Lexis-Nexis Academic Universe database, 4,889 (39 percent) voluntarily disclose balance sheet information. However, investors may also have access to balance sheet information through conference calls. Frankel et al. (1999) report that 85 percent of conference calls relate to quarterly earnings announcements. Ultimately, however, we cannot be sure that balance sheet information is available at the earnings announcement date for firms that do not voluntarily disclose this information in their earnings announcements.

Data

Our sample period spans 1992 through 1995 and all observations have stock price information in the CRSP database, earnings forecasts in First Call, Compustat data to compute abnormal working capital accruals, and the adjustment factor for stock dividends and splits in First Call or the quarterly Compustat database.[10] To precisely capture the announcement date, sample firms must have *Wall Street Journal* announcement dates (in the *Wall Street Journal Abstracts* in *Proquest*) exactly one trading day after the wire service/press release announcement dates (in First Call). Finally, we delete observations in the top and bottom 1 percent of returns and forecast errors to mitigate the effects of potential outliers. This selection procedure yields a sample of 14,839 firm-quarter observations.

We measure earnings expectations using the most recent mean analyst forecast from First Call. First Call updates on a real-time basis by revising their reported mean and median forecasts whenever analysts revise their forecasts, reducing the error in our earnings surprise measure. The mean (median) time between our sample firm forecast dates and their *Wall Street Journal* earnings announcement dates is 15 (9) trading days with an upper quartile of 20 trading days and a lower quartile of 5 trading days. We measure the earnings surprise as the difference between First Call reported earnings per share and First Call mean earnings forecast per share. First Call defines forecasts and earnings realizations as earnings from continuing operations, so they exclude transitory items such as special and extraordinary items. Thus, transitory items do not confound our earnings surprise measure.

Table 1 presents descriptive statistics. Panel A indicates that our sample observations have zero median forecast errors, and slightly positive abnormal returns and abnormal working capital accruals. Panel B presents mean values of our variables of interest partitioned by the sign of the earnings surprise and the sign of abnormal accruals. The last row

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[10] We use First Call split factors to adjust all variables to an equivalent per-share basis at the time of the earnings announcement. However, some split factors in earlier periods are missing. The director of quantitative research at First Call confirmed that the split factors in First Call are essentially identical to those in the quarterly Compustat database. Thus, when a firm's split factors are missing in First Call, we use Compustat split factors.

TABLE 1
Descriptive Statistics

Panel A: Distribution of Cumulative Abnormal Returns, Earnings Forecast Errors, and Abnormal Accruals (n = 14,839)

Variable	*Mean*	σ	*Lower Quartile*	*Median*	*Upper Quartile*
Cumulative abnormal returns (CAR)[a]	0.001	0.054	−0.028	0.001	0.023
Earnings forecast error per share, scaled by share price (FE/P_{-2})[b]	−0.001	0.007	−0.001	0.000	0.001
Abnormal working capital accruals per share, scaled by price ($AWCA/P_{-2}$)[c]	0.001	0.116	−0.021	0.002	0.028

Panel B: Mean Values of Variables of Interest by Sign of Earnings Surprise and Sign of Abnormal Accruals

			Good News Earnings Surprises[d]		*Bad News Earnings Surprises*[e]	
	Total Sample	*Zero Forecast Error*	*Income-Decreasing Working Capital Accruals*	*Income-Increasing Working Capital Accruals*	*Income-Decreasing Working Capital Accruals*	*Income-Increasing Working Capital Accruals*
CAR	0.001	−0.001	0.019	0.012	−0.009	−0.016
FE/P_{-2}	−0.001	0.000	0.002	0.002	−0.006	−0.006
$AWCA/P_{-2}$	0.001	0.003	−0.048	0.041	−0.066	0.065
N	14,839	3,303	3,098	2,762	2,470	3,206

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[a] CAR = Market-adjusted stock returns accumulated over the trading days [−1,0], where 0 is the *Wall Street Journal* earnings announcement date. Earnings announcement dates are obtained from the *Wall Street Journal Abstracts* and are required to be exactly one trading day after the wire service/press release announcement date (from First Call).

[b] FE/P_{-2} = Actual earnings for the current quarter minus the most recent mean analysts' forecast of current quarter earnings reported by First Call, scaled by closing share price at trading day −2.

[c] $AWCA_t = WC_t - [(WC_{t-4}/S_{t-4}) \times S_t]$

where:

t = year-quarter; thus $t - 1$ refers to the prior quarter and $t - 4$ refers to the same quarter in the prior year;

$AWCA_t$ = abnormal working capital accruals in the current quarter;

WC_t = working capital in the current quarter where working capital is computed as (current assets − cash and short-term investments) − (current liabilities − short-term debt);

WC_{t-4} = working capital in the same quarter last year;

S_t = sales in the current quarter, and

S_{t-4} = sales in the same quarter last year.

[d] Good news earnings surprises are defined as actual earnings exceeding forecasted earnings for the quarter.

[e] Bad news earnings surprises are defined as actual earnings falling short of forecasted earnings for the quarter.

indicates that our observations are reasonably well distributed over the four partitions. Consistent with prior research, the good news surprise partitions have positive CARs and the bad news surprise partitions have negative CARs. The mean forecast errors scaled by price (FE/P_{-2}) are identical across income-increasing and income-decreasing firms within each surprise partition. In addition, the mean abnormal working capital accruals scaled by price ($AWCA/P_{-2}$) are quite symmetric across the income-increasing and income-decreasing firms within each surprise partition.

IV. RESULTS

Hypotheses Tests

Table 2 presents our hypotheses tests. The first row of Panel A reports that the unconditioned ERC is 1.38, somewhat larger than the ERCs in prior research with similar return periods. Using First Call real-time forecasts and restricting our sample to firms with earnings announcements within one day of the *Wall Street Journal* announcement likely yields more precise forecast errors and probably explains our larger ERCs.[11]

The results in the second row of Panel A support our predictions. The coefficient on the good news income-increasing partition is negative and the coefficient on the bad news income-increasing partition is positive, both at the 1 percent level (one-tailed).[12] Panel B presents the regression coefficients in a 2 × 2 table based on the signs of the forecast errors and the abnormal accruals. As documented in Basu (1997, 23), ERCs are asymmetric with respect to the sign of the surprise. Good news earnings surprises are associated with higher ERCs (Basu [1997] attributes this to greater persistence for income-increasing surprises). We find that the ERCs are also asymmetric with respect to the sign of the abnormal accruals within each surprise partition. This is consistent with the income-increasing abnormal accruals exaggerating the magnitude of the good news and suppressing the magnitude of the bad news, and the income-decreasing abnormal accruals suppressing the magnitude of the good news and exaggerating the magnitude of the bad news.

We also note that abnormal accruals suppressing earnings surprises are consistent with "income-smoothing." However, our results suggest the market does not consistently reward or punish income smoothing. While higher ERCs in the cell with *good* news surprises and income-*decreasing* abnormal accruals are consistent with the market rewarding smoothing, higher ERCs in the cell with *bad* news surprises and income-*increasing* abnormal accruals are consistent with the market punishing smoothing.

Hypotheses Tests with Controls

Because prior research finds that ERCs are associated with size, risk, growth and earnings persistence (Easton and Zmjewski 1989; Collins and Kothari 1989), we rerun our analysis after including controls for the number of analysts, firm size, the market-to-book ratio, growth in book value of equity, earnings volatility, and earnings persistence.[13] Descriptive statistics for the control variables appear in Table 3, Panel A. We perform the

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[11] The standard deviation of our forecast errors reported in Table 1 is smaller than generally reported in previous studies. For example, Philbrick and Ricks (1991, Panel A, Table 1) report that I/B/E/S-based forecast errors (scaled by price) have the smallest standard deviation (0.051), which is much larger than our standard deviation of 0.007.

[12] Throughout the paper we report one-tailed significance levels when our predictions are signed and two-tailed significance levels otherwise. Clustering in event time or industry is unlikely to affect our inferences, because less than 1 percent of our sample is clustered on the same event date, and the most frequent industry represents less than 9 percent of our observations.

[13] Earnings persistence is measured as $(1 - \theta)$ with the assumption that annual earnings per share (adjusted for stock dividends and splits) follows an IMA(1,1) process: i.e., $EPS_y = EPS_{y-1} + \varepsilon_y - \theta\varepsilon_{y-1}$. While interest rates are also expected to impact ERCs, we assume that interest rates do not fluctuate significantly over the period of our analysis.

TABLE 2

Regression of Two-Day Market-Adjusted Returns on Earnings Forecast Errors Partitioned by Good/Bad Earnings News and Income-Increasing/Decreasing Abnormal Working Capital Accruals

(n = 14,839)

Panel A: Regression Analysis[a]

$$CAR = \alpha + \beta_1(GOODNEWS \times FE/P_{-2}) + \beta_2(GOODNEWSINCR \times FE/P_{-2}) + \beta_3(BADNEWS \times FE/P_{-2}) + \beta_4(BADNEWSINCR \times FE/P_{-2}) + \varepsilon$$

			Good News Earnings Surprises		*Bad News Earnings Surprises*		
	Intercept	FE/P_{-2}	*Good News Dummy* × FE/P_{-2}	*Income-Increasing Dummy* × FE/P_{-2}	*Bad News Dummy* × FE/P_{-2}	*Income-Increasing Dummy* × FE/P_{-2}	*Adj.* R^2
Predictions		+	+	−	+	+	
Coefficient	0.00	1.38					0.027
t-statistic	(5.98)***	(20.39)***					
Coefficient	−0.00		6.05	−1.47	0.57	0.53	0.046
t-statistic	(−3.51)***		(19.16)***	(−3.22)***	(5.48)***	(3.83)***	
n for dummy = 1			[5,860]	[2,762]	[5,676]	[3,206]	

(Continued on next page)

TABLE 2 (Continued)

Panel B: Earnings Response Coefficients from Panel A Partitioned by Sign of Earnings Surprise and Sign of Abnormal Accruals

		Earnings Surprise				*Differences (GOOD NEWS Minus BAD NEWS Cells)*
		GOOD NEWS		*BAD NEWS*		
Abnormal Working Capital Accruals	Income-Increasing	*(i)*	4.58***	*(ii)*	1.10***	3.48***
	Income-Decreasing	*(iii)*	6.05***	*(iv)*	0.57***	5.48***
	Hypotheses Tests: (Income-Increasing minus Income-Decreasing cells)					
	Prediction		−		+	
			−1.47***		0.53***	

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*, **, *** $p < 0.10$, $p < 0.05$, $p < 0.01$, respectively, one-tailed.

[a] The variables in the regression are defined as follows:

CAR = market-adjusted stock returns accumulated over the two trading days [−1,0] where 0 is the *Wall Street Journal* earnings announcement date. Earnings announcement dates are obtained from the *Wall Street Journal Abstracts* and are required to be exactly one trading day after the wire service/press release announcement date (from First Call);

FE/P_{-2} = actual earnings for the current quarter minus the most recent mean analysts' forecast of current quarter earnings reported by First Call, scaled by closing share price at trading day −2;

GOODNEWS = good news dummy that equals 1 if actual earnings exceed forecast;

GOODNEWSINCR = good news income-increasing dummy that equals 1 if GOODNEWS equals 1 and abnormal working capital accruals increase income;

BADNEWS = bad news dummy that equals 1 if actual earnings fall short of forecast;

BADNEWSINCR = bad news income-increasing dummy that equals 1 if BADNEWS equals 1 and abnormal working capital accruals increase income; and

ε = error term.

TABLE 3

Regression of Two-Day Market-Adjusted Returns on Earnings Forecast Errors Partitioned by Good/Bad Earnings News and Income-Increasing/Decreasing Abnormal Working Capital Accruals, with Control Variables[a]

Panel A: Univariate Statistics for Control Variables

		GOOD NEWS Earnings Surprises			*BAD NEWS Earnings Surprises*		
		Income- decreasing *Working Capital Accruals*	*Income-* increasing *Working Capital Accruals*	*Difference*	*Income-* decreasing *Working Capital Accruals*	*Income-* increasing *Working Capital Accruals*	*Difference*
Number of analysts	Mean	6.5	6.2	0.3**	5.9	5.8	0.1
	Median	5	4	1**	4	4	0
	n	3,098	2,762		2,470	3,206	
Size ($millions)	Mean	2,105	1,966	139	1,538	1,395	143
	Median	439	390	49***	282	236	46***
	n	3,096	2,755		2,465	3,193	
Market-to-book equity ratio	Mean	3.43	3.48	−0.05	2.79	3.19	0.40
	Median	2.47	2.54	−0.07*	1.96	2.03	0.07
	n	3,023	2,693		2,395	3,126	
Growth in book value of equity	Mean	0.04	−0.07	0.11	−0.02	0.13	−0.15
	Median	0.10	0.11	−0.01***	0.09	0.11	−0.02***
	n	2,299	1,805		1,847	2,191	
Earnings volatility	Mean	7.25	15.20	−7.95	18.79	3.75	15.04**
	Median	0.0057	0.0060	−0.0003**	0.0063	0.0059	0.0004
	n	2,321	1,860		1,867	2,243	
Earnings persistence	Mean	0.92	0.88	0.04**	0.79	0.77	0.02
	Median	0.92	0.87	0.05**	0.72	0.69	0.03
	n	1,815	1,439		1,454	1,721	

(*Continued on next page*)

TABLE 3 (Continued)

Panel B: Regression Analysis ($n = 7{,}849$)

$$CAR = \alpha + \beta_1(GOODNEWS \times FE/P_{-2}) + \beta_2(GOODNEWSINCR \times FE/P_{-2}) + \beta_3(BADNEWS \times FE/P_{-2})$$
$$+ \beta_4(BADNEWSINCR \times FE/P_{-2}) + \beta_5(Number\ of\ analysts\ dummy \times FE/P_{-2}) + \beta_6(Size\ dummy \times FE/P_{-2})$$
$$+ \beta_7(Market\text{-}to\text{-}book\ dummy \times FE/P_{-2}) + \beta_8(Growth\ dummy \times FE/P_{-2}) + \beta_9(Earnings\ volatility\ dummy \times FE/P_{-2})$$
$$+ \beta_{10}(Earnings\ persistence\ dummy \times FE/P_{-2}) + \varepsilon$$

		GOOD NEWS Earnings Surprises		*BAD NEWS Earnings Surprises*								
	Intercept	*Good News Dummy* × FE/P_{-2}	*Income-Increasing Dummy* × FE/P_{-2}	*Bad News Dummy* × FE/P_{-2}	*Income-Increasing Dummy* × FE/P_{-2}	*Number of Analysts Dummy* × FE/P_{-2}	*Firm Size Dummy* × FE/P_{-2}	*Market-to-Book Equity Ratio Dummy* × FE/P_{-2}	*Growth in Book Value of Equity Dummy* × FE/P_{-2}	*Earnings Volatility Dummy* × FE/P_{-2}	*Earnings Persist-ence Dummy* × FE/P_{-2}	*Adj.* R^2
Prediction		+	−	+	+	?	?	+	+	−	+	
Coefficient	−0.00	6.07	−1.62	0.52	0.64	0.16	0.11	0.70	0.60	−0.61	0.22	0.059
t-statistic	(−2.81)***	(13.24)***	(−2.74)***	(2.10)***	(3.22)***	(0.71)	(0.45)	(2.77)***	(2.98)***	(−2.95)***	(1.12)	
n for Dummy = 1		[3,139]	[1,389]	[3,052]	[1,661]							

(*Continued on next page*)

TABLE 3 (Continued)

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*, **, *** $p < 0.10$, $p < 0.05$, $p < 0.01$, one-tailed for predicted signs in Panel B, all others are two-tailed, respectively, where p-values corresponding to differences in means refer to t-tests, and p-values corresponding to differences in medians refer to Wilcoxon two-sample tests.

[a] The variables are defined as follows:

CAR = market-adjusted stock returns accumulated over the trading days [−1,0], where 0 is the *Wall Street Journal* earnings announcement date. Earnings announcement dates are obtained from the *Wall Street Journal Abstracts* and are required to be exactly one trading day after the wire service/press release announcement date (from First Call);

FE/P_{-2} = actual earnings for the current quarter minus the most recent mean analysts' forecast of current quarter earnings reported by First Call, scaled by closing share price at trading day −2;

GOODNEWS = good news dummy that equals 1 if actual earnings exceed forecast;

GOODNEWSINCR = good news income-increasing dummy that equals 1 if GOODNEWS equals 1 and abnormal working capital accruals increase income;

BADNEWS = bad news dummy that equals 1 if actual earnings fall short of forecast;

BADNEWSINCR = bad news income-increasing dummy that equals 1 if BADNEWS equals 1 and abnormal working capital accruals increase income;

Number of analysts dummy = 1 if the number of analysts' forecasts from the First Call database is above the sample median and 0 otherwise;

Size dummy = 1 if the market value of the firm at the end of the quarter is above the sample median and 0 otherwise;

Market-to-book equity ratio dummy = 1 if the market value of common equity divided by the book value of common equity at the end of the quarter is above the sample median and 0 otherwise;

Growth in book value of equity dummy = 1 if the average growth in the book value of equity over the prior three years is above the sample median and 0 otherwise;

Earnings volatility dummy = 1 if the variance of ROE over the past five years (where ROE is defined as earnings before extraordinary items divided by beginning of year equity) is above the sample median and 0 otherwise;

Earnings persistence dummy = 1 if $(1 - \theta)$ is above the sample median and 0 otherwise, with the assumption that annual earnings per share (adjusted for stock dividends and splits) follows an IMA(1,1) process: i.e., $EPS_y = EPS_{y-1} + \varepsilon_y - \theta\, \varepsilon_{y-1}$. We use earnings per share data from 1987 to 1996 to estimate persistence; and

ε = error term.

regression analysis with 7,849 firm-quarter observations that have complete data for all control variables, but we present descriptive statistics using all available observations. Although differences between income-increasing and income-decreasing firms within each surprise partition are modest, some are statistically significant. In particular, the median firm size is larger and the median growth in book value is smaller, among the income-decreasing observations.

The results in Panel B of Table 3 indicate that our hypotheses are supported after controlling for the variables in Panel A. The coefficient for the good news income-increasing partition remains negative and the coefficient for the bad news income-increasing partition remains positive, both at the 1 percent level (one-tailed).[14]

Forecast Revision Tests

If our hypotheses are true, then we also expect securities analysts to consider the reversing implications of abnormal accruals when revising future forecasts in response to current period earnings surprises. Good news forecast errors that contain income-increasing abnormal accruals should result in smaller upward forecast revisions than good news forecast errors that contain income-decreasing abnormal accruals. Similarly, bad news forecast errors that contain income-increasing abnormal accruals should result in larger downward forecast revisions than bad news forecast errors that contain income-decreasing abnormal accruals. We test this conjecture by estimating the parameters in the following model.

$$\begin{aligned} FR_{t+1}/P_{-2} = \alpha &+ \beta_1(\text{GOODNEWS} \times FE/P_{-2}) + \beta_2(\text{GOODNEWSINCR} \times FE/P_{-2}) \\ &+ \beta_3(\text{BADNEWS} \times FE/P_{-2}) + \beta_4(\text{BADNEWSINCR} \times FE/P_{-2}) + \varepsilon \qquad (3) \end{aligned}$$

where:

FR_{t+1}/P_{-2} = the first analyst forecast revision of next quarter's earnings after the current period's earnings announcement, computed as the revised forecast minus the original forecast, scaled by the closing share price at trading day −2.

The other variables are as defined previously.

We restrict this analysis to forecast revisions within 40 days following the *Wall Street Journal* earnings announcement, resulting in a mean (median) of 8.7 (4) trading days between the earnings announcement and the revision, with an upper quartile of 12 days and a lower quartile of one day. Consistent with our hypotheses, Table 4 reports that the coefficient on the good news income-increasing partition (β_2) is significantly negative, and the coefficient on the bad news income-increasing partition (β_4) is significantly positive, both at the 1 percent level (one-tailed). Therefore, this test provides additional evidence that market participants (in this case analysts) consider the reversing nature of abnormal accruals when interpreting earnings surprises.

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[14] We also (sequentially) test these alternative specifications: (1) market-to-book ratio (which is measured at the end of the period in the Table 3 analysis) is replaced by market-to-book measured at the beginning of the period; (2) growth in book value of equity is replaced by growth in market capitalization measured as the average growth over the prior three years; (3) earnings volatility is replaced by return volatility measured as the variance in monthly returns over the prior 36 months; (4) IMA earnings persistence is replaced by earnings persistence calculated as the coefficient on an AR(1) process, following Ohlson (1995). The results continue to support our hypotheses as reported in Table 3.

TABLE 4

Regression of Analysts' Forecast Revisions on Earnings Forecast Errors Partitioned by Good/Bad Earnings News and Income-Increasing/Decreasing Abnormal Working Capital Accruals

(n = 8,561)[a]

$$FR_{t+1}/P_{-2} = \alpha + \beta_1(GOODNEWS \times FE/P_{-2}) + \beta_2(GOODNEWSINCR \times FE/P_{-2}) + \beta_3(BADNEWS \times FE/P_{-2}) + \beta_4(BADNEWSINCR \times FE/P_{-2}) + \varepsilon$$

		GOOD NEWS Earnings Surprises		*BAD NEWS Earnings Surprises*		
	Intercept	*Good News Dummy* × *FE/P_{-2}*	*Income-Increasing Dummy* × *FE/P_{-2}*	*Bad News Dummy* × *FE/P_{-2}*	*Income-Increasing Dummy* × *FE/P_{-2}*	*Adj. R^2*
Predictions		+	−	+	+	
Coefficient	−0.00	0.30	−0.11	0.24	0.10	0.165
t-statistic	(−16.42)***	(12.00)***	(−3.03)***	(19.94)***	(6.39)***	
n for dummy = 1		[3,627]	[1,708]	[3,257]	[1,902]	

*, **, *** $p < 0.10$, $p < 0.05$, $p < 0.01$, respectively, one-tailed.

[a] The variables in the regression are defined as follows:

FR_{t+1}/P_{-2} = the first analyst forecast revision of next quarter's earnings made after the current period's earnings announcement, computed as the revised forecast minus the original forecast, closing share price at trading day −2;

FE/P_{-2} = actual earnings for the current quarter minus the most recent mean analysts' forecast of current quarter earnings reported by First Call, scaled by closing share price at trading day −2;

GOODNEWS = good news dummy that equals 1 if actual earnings exceed forecast;

GOODNEWSINCR = good news income-increasing dummy that equals 1 if GOODNEWS equals 1 and abnormal working capital accruals increase income;

BADNEWS = bad news dummy that equals 1 if actual earnings fall short of forecast;

BADNEWSINCR = bad news income-increasing dummy that equals 1 if BADNEWS equals 1 and abnormal working capital accruals increase income; and

ε = error term.

Post-Announcement Abnormal Returns

While the above analysis suggests that the market anticipates the reversing implications of abnormal accruals, it does not address whether the market fully impounds the implications over our two-day event window. We investigate this issue by examining the cumulative abnormal returns during the 80 trading days following the earnings announcement.[15]

Panel A of Table 5 reports post-announcement CARs, in ten-day increments, over the 80 days following the quarterly earnings announcements. At the end of the 80-day period, differences in cumulative abnormal returns across the accruals partitions are 2.4 percent for the good news surprises and 3.0 percent for the bad news surprises. Figure 1 displays the

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[15] Because we restrict this analysis to observations with complete returns data, we drop 22 and 15 (21 and 30) good (bad) news income-increasing and income-decreasing observations, respectively. Since this is less than 1 percent of each partition, survivorship bias is unlikely to affect our results.

TABLE 5

Cumulative Abnormal Returns (CARs), by Good/Bad Earnings News and Income-Increasing/Decreasing Partitions for +1 to +80 days Following the Earnings Announcement Date, for the Full Sample (Panel A) and for the Sample Matched on Earnings Announcement Dates (Panel B)[a]

Panel A: Full Sample

	GOOD NEWS Earnings Surprises			*BAD NEWS Earnings Surprises*		
	(1) Income-Decreasing Accruals	*(2) Income-Increasing Accruals*		*(3) Income-Decreasing Accruals*	*(4) Income-Increasing Accruals*	
Event Window	*n = 3,076*	*n = 2,747*	*Difference (1) − (2)*	*n = 2,449*	*n = 3,176*	*Difference (3) − (4)*
[+1,+10]	0.005	0.003	0.002	0.001	−0.001	0.002
[+1,+20]	0.010	0.007	0.003	0.003	0.002	0.001
[+1,+30]	0.010	0.007	0.003	−0.002	−0.004	0.002
[+1,+40]	0.012	0.005	0.007*	−0.006	−0.012	0.006
[+1,+50]	0.014	0.004	0.010**	−0.005	−0.020	0.015***
[+1,+60]	0.020	0.004	0.016***	−0.003	−0.027	0.024***
[+1,+70]	0.027	0.003	0.024***	−0.002	−0.030	0.028***
[+1,+80]	0.029	0.005	0.024***	0.002	−0.028	0.030***

(*Continued on next page*)

TABLE 5 (Continued)

Panel B: Sample Matched on Earnings Announcement Dates[b]

	GOOD NEWS Earnings Surprises			*BAD NEWS Earnings Surprises*		
Event Window	*(1) Income-Decreasing Accruals* $n = 580$	*(2) Income-Increasing Accruals* $n = 580$	*(1) − (2)*	*(3) Income-Decreasing Accruals* $n = 559$	*(4) Income-Increasing Accruals* $n = 559$	*(3) − (4)*
[+1,+10]	0.007	0.004	0.003	−0.003	0.000	−0.003
[+1,+20]	0.013	0.011	0.002	0.000	0.002	−0.002
[+1,+30]	0.008	0.010	−0.002	−0.005	−0.005	0.000
[+1,+40]	0.012	0.007	0.005	−0.009	−0.012	0.003
[+1,+50]	0.016	0.008	0.008	−0.007	−0.013	0.006
[+1,+60]	0.022	0.007	0.015	−0.005	−0.019	0.014
[+1,+70]	0.032	0.003	0.029**	0.003	−0.016	0.019*
[+1,+80]	0.036	0.010	0.026**	0.006	−0.019	0.025**

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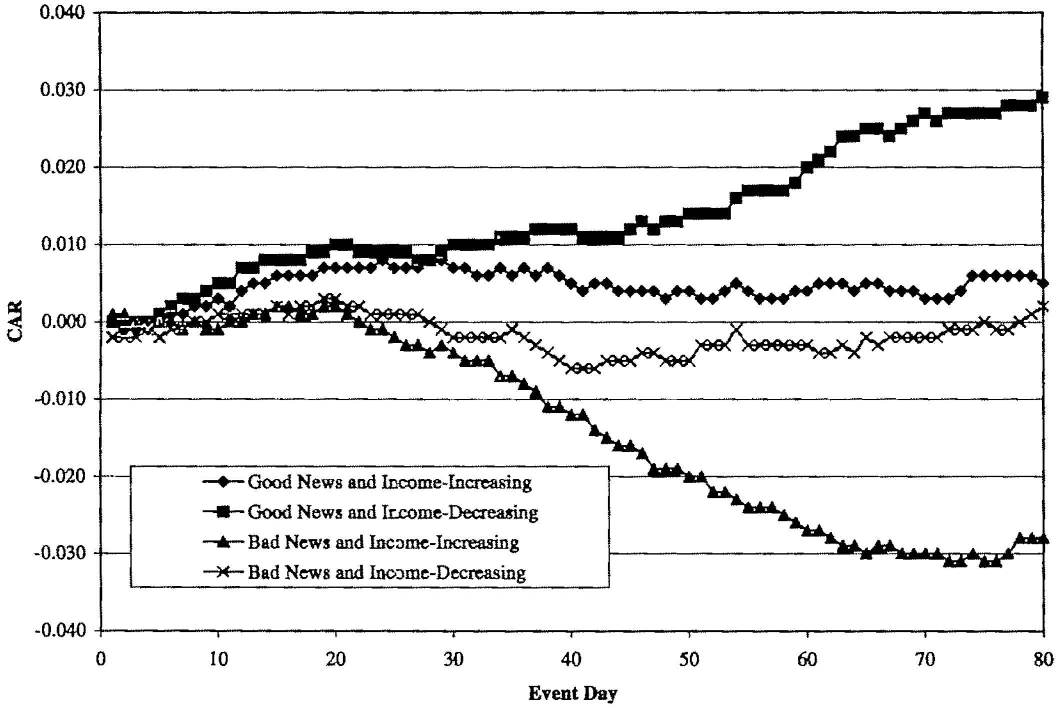
*, **, *** $p < 0.10$, $p < 0.05$, $p < 0.01$, respectively, two-tailed.

[a] Cumulative abnormal returns are market-adjusted and begin cumulating on the day after the *Wall Street Journal* announcement date. Earnings surprises and working capital accruals are defined in Table 1.

[b] Observations are pair-wise matched such that an income-increasing and an income-decreasing observation occur on the same earnings announcement date. When more than one match occurs on a given announcement date, we retain the observation with the smallest absolute difference in abnormal accruals.

FIGURE 1
Cumulative Abnormal Returns (CARs) by Signs of the Earnings Surprise and the Abnormal Accruals for Event Window [+1, +80]



CARs from Table 5 and shows that the good news income-decreasing observations increase in value, while the bad news income-increasing firms decline in value. This is consistent with the market under-anticipating both the positive implications of income-decreasing abnormal accruals and the negative implications of income-increasing abnormal accruals.

The analysis in Table 5 suggests that the market underestimates the magnitude of the pricing implications of the abnormal accruals. To quantify the market's underestimation, we calculate the relative proportion of the pricing implications that the market assimilates at the earnings announcement date. Table 5 reports that *net* abnormal returns of 2.4 percent and 3.0 percent accrue to the good and bad news partitions, respectively, during the 80 days after the earnings announcement. Panel B of Table 1 suggests that the market assimilates a 0.7 percent *net* abnormal return during the two-day [−1,0] announcement period for both the good news partition (0.019 minus 0.012) and the bad news partition (−0.009 minus −0.016). This suggests that market participants anticipate approximately 23 percent [0.7 percent/(0.7 percent + 2.4 percent)] and 19 percent [0.7 percent/(0.7 percent + 3.0 percent)] of the pricing implications of the abnormal accruals in the good and bad news partitions, respectively. In supplementary analysis we also examine abnormal returns over the subsequent 250 trading days (approximately one year) and find that *net* abnormal returns of 5.3 percent accrue to each of the good and bad news partitions. This suggests that the market anticipates approximately 12 percent of the one-year returns [0.7 percent/(0.7 percent + 5.3 percent)].

The analysis in Panel A of Table 5 does not represent a zero net investment portfolio because earnings announcement dates for the income-increasing observations do not necessarily correspond with the announcement dates for the income-decreasing observations. Therefore, we replicate this analysis after pair-wise matching the income-increasing and income-decreasing observations on announcement dates (following Bernard and Thomas 1989). The results appear in Panel B of Table 5.[16] While matching reduces the number of observations to less than 600 in each partition, the results at the end of the 80-day period are quite similar to those reported in Panel A. Thus, restricting the post-announcement analysis to a zero net investment portfolio does not alter our conclusions.

Another limitation of the analysis in Panel A of Table 5 is that it does not control for risk factors that may affect post-announcement returns. We address this issue in two ways. First, we run Fama and MacBeth (1973) regressions over the 40-, 60-, and 80-day periods following the quarterly earnings announcements. This regression includes dummy variables capturing the returns for each of our partitions and continuous control variables for Fama and French (1992) risk factors. The control variables are firm size (log of market value), book-to-market (log form), beta (estimated over the 250 days ending two days before the earnings announcement), and the earnings-to-price ratio (prior year's annual earnings per share divided by end of the prior year stock price). We run individual regressions for each of the 16 quarters during the four-year period of our analysis and present the means of the 16 estimated coefficients along with t-statistics computed from their time-series standard error (as suggested in Bernard 1987). Panel A of Table 6 presents the regression statistics after controlling for the risk factors and Panel B presents test statistics for the income-decreasing minus the income-increasing returns for each earnings surprise partition. While Panel A indicates that the returns for the individual partitions differ somewhat from those estimated in Panel A of Table 5, the differences reported in Panel B of Table 6 are generally consistent with those found in Table 5, particularly over the entire 80-day period. Thus, risk differences do not appear to explain our inference that the market does not fully assimilate the pricing implications of abnormal accruals.

Bernard et al. (1997) suggest that abnormal returns clustered around subsequent earnings announcements are more likely due to informational mispricing than omitted risk factors. Consequently, we also examine returns at the next quarter's earnings announcement. As in prior literature we examine returns during the three-day window $[-2,0]$ ending with the Compustat earnings announcement date (Bernard and Thomas 1990; Sloan 1996; Collins and Hribar 2000). The results in Table 6, Panel C, indicate that while the three-day window represents only 3.8 percent of the 80-day accumulation period, it contains 21 percent (0.5/2.4) and 13 percent (0.4/3.0) of the net abnormal returns contained in the good and bad news partitions, respectively, as reported in Panel A of Table 5.[17]

Taken together, the analyses in Tables 5 and 6 suggest that the market, on average, does not fully impound the pricing implications of abnormal working capital accruals associated with earnings surprises. The purpose of our investigation is to assess whether market participants fully understand the pricing implications of abnormal accruals, not to identify an implementable strategy for capturing abnormal trading profits. In addition to risk factors, other barriers to earning excess trading profits include: (1) transactions costs

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[16] When more than one match occurs on a given announcement date, we retain the observation with the smallest absolute difference in abnormal accruals.

[17] Abnormal returns around earnings announcements may coincide with risk shifts, because *investment risk* is systematically related to *earnings changes* (Ball and Kothari 1991; Ball et al. 1993). However, it is unclear how *investment risk* is systematically related to *abnormal accruals* (after controlling for unexpected earnings).

TABLE 6
Cumulative Abnormal Returns with Controls for Risk Factors (Panels A and B), and Returns during the Three-Day Window around Subsequent Earnings Announcements (Panel C)

Panel A: Cross-Sectional Regressions with Controls for Risk Factors (n = 14,731)[a]

$$CAR = \alpha + \beta_1(GOODNEWSINCR) + \beta_2(GOODNEWSDECR) + \beta_3(BADNEWSINCR) + \beta_4(BADNEWSDECR) + \beta_5(Firm\ Size) + \beta_6(Book\text{-}to\text{-}Market) + \beta_7(Stock\ Beta) + \beta_8(Earnings\text{-}to\text{-}Price\ Ratio) + \varepsilon$$

	Cumulative Abnormal Returns (CARs)		
	[+1,+40]	*[+1,+60]*	*[+1,+80]*
Intercept	0.026	0.044	0.055
(t-stat)	(2.83)**	(3.57)***	(4.33)***
Good News Surprises			
Income-increasing dummy	0.007	0.005	−0.002
(t-statistic)	(1.20)	(0.74)	(−0.23)
Income-decreasing dummy	0.013	0.018	0.019
(t-statistic)	(4.43)***	(3.56)***	(3.29)***
Bad News Surprises			
Income-increasing dummy	−0.016	−0.034	−0.043
(t-statistic)	(−5.13)***	(−8.01)***	(−7.64)***
Income-decreasing dummy	−0.010	−0.012	−0.016
(t-statistic)	(−2.92)**	(−2.17)**	(−2.21)**
Risk Factor Controls			
Firm size	−0.003	−0.005	−0.005
(t-statistic)	(−2.04)*	(−2.73)**	(−2.85)**
Book-to-market equity ratio	0.005	0.007	0.009
(t-statistic)	(1.35)	(1.34)	(1.41)
Stock beta	−0.004	−0.004	0.000
(t-statistic)	(−0.87)	(−0.71)	(0.01)
Earnings-to-price ratio	−0.007	−0.021	−0.008
(t-statistic)	(−0.35)	(−0.68)	(−0.20)

Panel B: Differences in Returns between the Abnormal Accruals Partitions Estimated in Panel A

	[+1,+40]	*[+1,+60]*	*[+1,+80]*
Differences in Good News Partitions			
Income-decreasing –	0.006	0.013	0.021
Income-increasing	(0.99)	(1.45)	(2.28)**
Differences in Bad News Partitions			
Income-decreasing –	0.006	0.022	0.027
Income-increasing	(1.40)	(3.23)***	(3.05)***

(*Continued on next page*)

TABLE 6 (Continued)

Panel C: Abnormal Returns during Three-Day Window around Subsequent Earnings Announcements

	GOOD NEWS Earnings Surprises			*BAD NEWS Earnings Surprises*		
Event Window	*(1) Income-Decreasing Accruals n = 2,905*	*(2) Income-Increasing Accruals n = 2,566*	*(1) − (2)*	*(3) Income-Decreasing Accruals n = 2,240*	*(4) Income-Increasing Accruals n = 2,859*	*(3) − (4)*
Cumulative abnormal return over three-day window around subsequent earnings announcements	0.008	0.003	0.005***	0.002	−0.002	0.004**

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*, **, *** $p < 0.10$, $p < 0.05$, $p < 0.01$, respectively, two-tailed.

[a] We run individual regressions for each of the 16 quarters during 1992–1995, and Panel A presents the means of the 16 estimated coefficients along with t-statistics from their time-series standard error. The variables in the regression are defined as follows:

CAR = market-adjusted stock returns accumulated over the trading days [−1,0], where 0 is the *Wall Street Journal* earnings announcement date. Earnings announcement dates are obtained from the *Wall Street Journal Abstracts* and are required to be exactly one trading day after the wire service/press release announcement date (from First Call);
GOODNEWSINCR = good news income-increasing dummy that equals 1 if GOODNEWS equals 1 and abnormal working capital accruals increase income;
GOODNEWSDECR = good news income-decreasing dummy that equals 1 if GOODNEWS equals 1 and abnormal working capital accruals decrease income;
BADNEWSINCR = bad news income-increasing dummy that equals 1 if BADNEWS equals 1 and abnormal working capital accruals increase income;
BADNEWSDECR = bad news income-decreasing dummy that equals 1 if BADNEWS equals 1 and abnormal working capital accruals decrease income;
Firm size = the natural log of the market value of the firm at the end of the quarter;
Book-to-market equity ratio = the natural log of the ratio of the book value of common equity divided by the market value of common equity at the end of the quarter;
Stock beta = estimated from a regression of daily raw returns on the CRSP value-weighted daily return index using the 250-day return period ending the two trading days before the *Wall Street Journal* date;
Earnings-to-price ratio = annual earnings per share of the prior year divided by the stock price at the end of the prior year; and
ε = error term.

of identifying and trading the firms in our partitions, (2) pricing pressure from enacting a trading rule, and (3) availability of balance sheet data to compute abnormal accruals. If balance sheet information is unavailable until the current quarter's 10-Q or 10-K is ultimately released, then the "post-announcement" returns are simply perfect foresight returns to the delayed release of the balance sheet information. However, if the full response occurs at the 10-Q or 10-K filing, then we would not find returns clustered around the subsequent quarter's earnings announcements as reported in Panel C of Table 6. Moreover, replicating our analyses in Tables 5 and 6 using just the 4,889 observations that voluntarily disclose balance sheet information with their earnings announcements yields inferences identical to those drawn based on the full sample.

V. ROBUSTNESS CHECKS

The study's hypotheses are still supported after the following robustness checks:

1) Tests allowing for a nonlinear earnings-return relation—Freeman and Tse (1992) find that the earnings-return relation is nonlinear and may be better specified using an arc-tangent regression model, so we rerun the regression in Table 2 using an arc-tangent specification.
2) Eliminating loss firms—Because loss firms' ERCs are on average lower than ERCs of firms with positive earnings (Hayn 1995) we rerun the regression in Table 2 after deleting loss firm observations.
3) Including intercepts for good and bad news—We rerun the analysis in Table 2 after including good and bad news dummy variables.
4) Including partitions based on sales growth—Because our measure of abnormal accruals is a function of sales in the current and prior year, we rerun the regression in Table 2 after adding variables that partition the forecast errors based on sales growth. We include a dummy for good news firms with low sales growth (below the median) times the forecast error, and a dummy for bad news firms with low sales growth (below the median) times the forecast error.
5) Eliminating observations with management forecasts—An alternative explanation for our findings is a market response to management earnings forecasts issued at the time of the earnings announcements. Thus, we rerun the regression in Table 2 after dropping observations that issue (or are suspected of issuing) management forecasts at the earnings announcement date, identified from the *Wall Street Journal Abstracts* in *Proquest* and First Call. Because First Call does not collect comprehensive data on management forecasts prior to 1995, we drop all observations for firms that issue at least one forecast during the period of our analysis, resulting in a loss of 5,360 observations.
6) Eliminating observations with large acquisitions or divestitures—Collins and Hribar (1999) report that acquisitions and divestitures may distort the interpretation of reported accounting accruals. Therefore, we rerun the analysis in Table 2 after dropping firms with acquisitions that exceed 5 percent of total sales and firms reporting discontinued operations (nonzero Compustat item #66) during the event year or the year preceding the event year. While the results continue to support our hypotheses, the significance of the coefficient on the bad news income-increasing partition is slightly weaker at $p < 0.02$ (one-tailed).
7) Alternative measure for working capital to sales ratio—We use prior year's same-quarter ratio of working capital to sales to capture the current quarter's expected ratio of working capital to sales. However, because the prior year's same quarter ratio of working capital to sales may include the initiation and/or reversal of abnormal accruals from prior periods, we rerun the regression in Table 2 using this ratio's average over both the prior two years, and prior three years. While the results of both regressions continue to support our hypotheses, the coefficients on the good news income-increasing partitions are significantly negative at $p \leq 0.04$ (one-tailed) or less.

In addition to the above robustness checks we also perform the following analyses.

Results by Quarter

We replicate the regression in Table 2 by fiscal quarter. The coefficient on the good news income-increasing dummy is significantly negative at the 10 percent level (one-tailed) or less in the first three quarters. The bad news income-increasing dummy is significantly positive at the 5 percent level (one-tailed) or less in the first three quarters. Our results therefore hold in all but the fourth quarter.

Alternative Accruals Models

Our abnormal working capital accruals model differs from the Jones-type models in several ways. However, it is an empirical question whether the two measures are empirically distinct. Therefore, we test whether Jones model discretionary accruals lead to similar inferences. We rerun our hypotheses tests in Table 2 after replacing our abnormal accruals measure with discretionary accruals calculated from the following working capital version of the quarterly Jones model (similar to Han and Wang 1998):

$$(WC_t - WC_{t-1}) = \alpha + \beta_1(S_t - S_{t-1}) + \beta_2 D1 + \beta_3 D2 + \beta_4 D3 + \Sigma DYear + \varepsilon \quad (4)$$

where:

WC_t = noncash working capital in the current quarter;
WC_{t-1} = noncash working capital in the previous quarter;
S_t = sales in the current quarter;
S_{t-1} = sales in the previous quarter;
D1 = dummy if observation relates to first quarter;
D2 = dummy if observation relates to second quarter;
D3 = dummy if observation relates to third quarter;
ΣDYear = dummies for years 1987 through 1995; and
ε = error term.

Abnormal working capital accruals are defined as the error term from equation (4). Consistent with Han and Wang (1998), we estimate the above model using pooled cross-sectional data, by industry.[18] We use two-digit industry SICs and firm quarter observations for the ten-year period from 1986–1995, requiring each quarter to have at least three observations over this period. We also eliminate influential observations using the procedure in Belsley et al. (1980), and delete the extreme 1 percent of the discretionary accruals estimates. This procedure results in 12,723 firm-quarter observations. The Pearson correlation coefficient between our measure of abnormal accruals and the Jones model accruals estimates is only 0.25. Given that our abnormal accruals measure is empirically quite distinct from the Jones model, it is not necessarily surprising that neither of our hypotheses is supported using the Jones model estimates.

We believe our abnormal accruals measure is more appropriate for our research question. While the Jones model estimates normal accruals based on a coefficient from a pooled cross-sectional regression (β_1) and includes the intercept term from this regression, our measure of normal accruals varies by quarter and year for each firm, and is thus specifically tailored to each observation in our sample.[19] Seasonal variations across industries suggest that quarter-specific measures are likely to better capture normal accruals. For example, a retailer's working-capital-to-sales ratio is likely to be different around the holiday season than in mid-summer. In addition, firm-specific measures are likely to be superior to industry-wide estimates. For example, within a given industry, a firm's size, age, and accounting choices may affect the normal level of working capital required to sustain current sales levels, but Jones model measures of normal accruals only reflect the average effects of these factors. Unfortunately, we are unable to identify the precise source of the different inferences obtained from the two models, and we leave this issue to future research.

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[18] We estimate the model separately for each industry in our sample, while Han and Wang (1998) examine only the oil and gas industry, and thus estimate one set of parameters for their study.

[19] While equation (4) includes quarter and year intercept terms, this simply removes the *average* variation across all quarters and years in the observations used to estimate the model, which is not equivalent to allowing β_1 to vary by quarter and year for each firm.

Our *abnormal* working capital accruals also differ from *total* working capital accruals. The Pearson correlation coefficient between our measure of abnormal accruals and total working capital accruals is only 0.30, indicating that the two measures are empirically quite distinct. Rerunning the Table 2 analysis after substituting total working capital accruals for our abnormal accruals measure reveals that our first hypothesis is not supported, but our second hypothesis is supported. These results are consistent with our measure of *abnormal* working capital accruals leading to more powerful tests than simply using *total* (normal plus abnormal) working capital accruals.

Analysis of Working Capital Accrual Components

To assess whether specific working capital components (such as current assets or current liabilities) drive our results, we rerun the regression in Table 2 partitioned on each of the following: current assets, current liabilities, inventories, and accounts receivable. Abnormal accruals for each component are computed as in equation (2), where working capital is replaced by the components. We find that our results are not concentrated in any single working capital component. The negative coefficient on the good news income-increasing partition appears largely attributable to either total current assets or total current liabilities, while the positive coefficient on the bad news income-increasing partition appears largely attributable to inventories and accounts receivable.

Fundamental Analysis Signals on Accounts Receivable and Inventory

Lev and Thiagarajan (1993) and Abarbanell and Bushee (1997, 1998) (hereafter LT and AB) find significant associations between fundamental signals and long window stock returns. Among the nine fundamental signals investigated in LT and AB, two capture aspects of working capital—the change in accounts receivable minus the change in sales, and the change in inventory minus the change in sales. Thus, we attempt to discern whether our partitions (based on our estimate of abnormal working capital accruals) have incremental explanatory power over partitions using the LT and AB measures. We rerun our analysis in Table 2 after adding forecast error partitions that use these two fundamental signals to capture income-increasing abnormal accruals. The results indicate that the coefficients on the partitions that capture our hypotheses tests are essentially unchanged. Thus, our abnormal working capital accruals measure appears distinct from the receivables and inventory measures in LT and AB.

Selective Balance Sheet Disclosure

Management may opportunistically guide market participants by selectively disclosing balance sheet information only when it contains income-*decreasing* abnormal accruals, because these accruals suppress underlying good news and exaggerate underlying bad news. However, the evidence does not support this conjecture since we find that 38.9 percent of the observations with income-*decreasing* abnormal accruals, and 39.6 percent of the observations with income-*increasing* abnormal accruals disclose balance sheet information.

Balance Sheet Data Availability

Our tests assume market participants have balance sheet information to compute working capital accruals. Therefore, we perform three additional analyses. First, we rerun the hypotheses tests in Table 2 using only the 4,889 observations with balance sheet disclosures included in their earnings announcements. We find that the coefficient on the good news income-increasing partition is insignificantly negative ($p = 0.19$, one-tailed), and the coefficient on the bad news income-increasing partition is positive at $p = 0.05$ (one-tailed). Low statistical power from dropping two-thirds of the sample is likely responsible for the weaker results.

Second, we rerun the forecast revision tests in Table 4 using only the 4,889 observations with voluntary balance sheet disclosures. The coefficient on the good news income-increasing partition is negative at $p = 0.09$ (one-tailed), and the coefficient on the bad news income-increasing partition is positive at $p < 0.01$ (one-tailed). Once again, the expected signs but weakened significance on the first hypothesis are likely due to loss of power.

Finally, we also rerun the hypotheses tests in Table 2, using the entire sample, after expanding the event window to include the 20 trading days (approximately one month) following the earnings announcement. Expanding the event window over this period assures that market participants are likely to have balance sheet information from the 10-Q or 10-K to compute working capital accruals. Our hypotheses continue to hold at $p < 0.01$ (one-tailed) in this analysis. Thus, our hypotheses are supported when we perform our tests over a period when balance sheet information is likely to be available for the entire sample.

Truncation of Abnormal Accruals Measure

Partitioning our data based on the sign of the abnormal accruals means that observations close to zero receive the same weight as observations that are far away from zero. However, we expect the observations farther from zero to have relatively greater pricing implications. Moreover, our noisy abnormal accruals measure is more likely to misclassify near-zero income-increasing observations as income-decreasing observations and vice versa. Thus, we rerun the Table 2 analysis after sequentially deleting quartiles of observations in each partition that are closest to zero. Despite the smaller number of observations, the (absolute) t-statistics become larger after dropping observations around zero. Thus, our inferences continue to hold in subsamples where our abnormal accruals measure is least noisy.

VI. SUMMARY

We hypothesize that the reversing implications of abnormal working capital accruals affect the market's perception of the underlying magnitude of earnings surprises. We test our hypotheses by examining ERCs for two-day event windows ending on the quarterly earnings announcement date. The evidence supports our hypotheses. Specifically, we find that the ERCs associated with good news earnings surprises that include income-*increasing* abnormal accruals are *lower* than ERCs associated with good news earnings surprises that include income-*decreasing* abnormal accruals. By contrast, the ERCs associated with bad news earnings surprises that include income-*increasing* abnormal accruals are ***higher*** when compared to ERCs associated with good news earnings surprises that include income-*decreasing* abnormal accruals.

Also consistent with our hypotheses, we find that analysts appear to consider the implications of abnormal accruals when revising future forecasts in response to current period earnings surprises. That is, analysts' forecast revisions following good news earnings surprises that include income-*increasing* abnormal accruals are *less* positive than forecast revisions following good news earnings surprises that include income-*decreasing* abnormal accruals. By comparison, analysts' forecast revisions following bad news earnings surprises that include income-*increasing* abnormal accruals are *more* negative than forecast revisions following good news earnings surprises that include income-*decreasing* abnormal accruals.

However, we also find that during the 80 trading days following the earnings announcement: (1) good news firms with income-decreasing abnormal accruals experience significantly higher cumulative abnormal returns than good news firms with income-increasing abnormal accruals, and (2) bad news firms with income-increasing abnormal accruals experience significantly lower cumulative abnormal returns than bad news firms with income-decreasing abnormal accruals. This suggests that the market does not fully assimilate the implications of abnormal accruals at the earnings announcement date.

Our findings are consistent with market participants using abnormal working capital accruals to interpret the underlying magnitude of earnings surprises, but not fully adjusting for their pricing implications. This is consistent with prior evidence that stock prices do not fully impound the implications of reported earnings (Foster et al. 1984; Freeman and Tse 1989; Bernard and Thomas 1990; Sloan 1996).

In addition, we estimate abnormal accruals using a model that, conceptually, attempts to capture the amount of working capital accruals that are unlikely to be sustained and, therefore, are expected to reverse against future earnings. Our empirical model of normal working capital accruals differs from the Jones-type models often used in prior research (Jones 1991) in some important respects. For example, while our model allows normal accruals to vary by quarter and year for each firm, and thus to be unique for each observation in our sample, the Jones-type models base their estimates on regression coefficients that do not allow for seasonality. Seasonal variations across industries suggest that quarter-specific measures likely result in superior accrual estimates in quarterly studies. Hence, not surprisingly, our findings are not replicated using a quarterly Jones-type model to estimate normal accruals. However, while we believe our model is more likely to capture abnormal accruals, we are unable to identify the precise source of the different inferences obtained from the two models, and we leave this issue to future research.

APPENDIX

Development of Abnormal Working Capital Accruals Measure

Our abnormal accruals measure attempts to capture the difference between reported working capital and the market's *expectations* of the normal working capital required to support current sales levels. This difference between realized working capital and the market's expectations of required working capital is the portion of working capital accruals that is expected to reverse against future earnings. This appendix formally develops our measure.

We define working capital accruals (WCA) as the change in non-cash working capital ($WC_t - WC_{t-1}$). Since we are interested in *quarterly* working capital, our time subscripts "t" refer to year-quarters. Thus, $t - 1$ refers to the quarter immediately preceding year-quarter t, and $t - 4$ refers to the same quarter in the prior year. We assume that the expectation of WCA is formed at time $t - \varepsilon$, just before market participants see working capital (WC_t), but after they observe $S_{t-\varepsilon}$, which we assume is equal to contemporaneous sales S_t. This essentially assumes that market participants first observe reported sales and then use that information to evaluate reported working capital. Thus, if I_{t-1} is the information available at time $t - 1$ we have:

$$\text{Unexpected WCA}_t = (WC_t - WC_{t-1}) - E_{t-\varepsilon}[(WC_t - WC_{t-1}) \mid I_{t-1}, S_{t-\varepsilon}] \quad \text{(A1)}$$

$$\text{Unexpected WCA}_t = WC_t - E_{t-\varepsilon}[WC_t \mid I_{t-1}, S_{t-\varepsilon}] \quad \text{(A2)}$$

In simplifying equation (A1) to equation (A2), beginning working capital (WC_{t-1}) drops out because WC_{t-1} is known at time $t - \varepsilon$, thus $E_{t-\varepsilon}[WC_{t-1}] = WC_{t-1}$. A reasonable expectation for working capital (WC_t) just after market participants have observed sales but before they have observed WC_t is the normal level of working capital required to support contemporaneous sales. Consistent with Dechow et al. (1998, 138, equation 9), we assume that sales follow a seasonal random walk and that *normal* working capital at time $t - \varepsilon$ is a fixed proportion of contemporaneous sales ($a_q S_{t-\varepsilon}$). Thus:

$$E_{t-\varepsilon}[WC_t \mid I_{t-1}, S_{t-\varepsilon}] = a_q S_{t-\varepsilon} \quad \text{(A3)}$$

We allow a_q to vary across quarters due to seasonality and empirically estimate seasonal a_q as the prior year's same-quarter ratio of working capital to sales (WC_{t-4}/S_{t-4}), which leads to:

$$E_{t-\varepsilon}[WC_t \mid I_{t-1}, S_{t-\varepsilon}] = (WC_{t-4}/S_{t-4})\, S_{t-\varepsilon}. \quad \text{(A4)}$$

After substituting equation (A4) into equation (A2), we derive the abnormal working capital accruals (AWCA) proxy we use in our tests:[20]

$$AWCA_t = WC_t - (WC_{t-4}/S_{t-4})\, S_{t-\varepsilon}. \quad \text{(A5)}$$

In summary, our abnormal accruals proxy in equation (A5) essentially captures the difference between realized working capital and the market's *expectation* of the normal level of working capital required to support current sales levels.

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[20] The terms unexpected and abnormal are often used interchangeably in the accounting literature. While the underlying construct for our measure is unexpected accruals, to be consistent with prior accruals research we label our measure abnormal accruals.

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The Effects of Domain Experience and Task Presentation Format on Accountants' Information Relevance Assurance

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ABSTRACT: Information relevance advisory services offer growth opportunities for accountants in CPA firms, but we know little about the types of knowledge needed to provide high-quality advice. In a two-stage experiment, accountants with different management and public accounting experiences (that we suggest lead to different types of knowledge) receive task information in alternative formats, and develop relevant information for a client's decision. We find that participants are more likely to choose an appropriate problem representation when they receive an appropriate task format or when they have more management or public accounting experience (stage one). Also, when participants choose an appropriate problem representation, more management accounting experience improves their development of relevant information, but more public accounting experience does not (stage two). Our results suggest that tailored task presentation and domain experience that

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facilitates acquisition of multiple knowledge types improve accountants' information relevance advice.

Keywords: *Information relevance assurance; domain experience; task presentation format; problem representation.*

Data Availability: *Contact the authors.*

I. INTRODUCTION

The American Institute of Certified Public Accountants' (AICPA) *CPA Vision Project—2011 and Beyond* (AICPA 1999a) identifies information assurance as a core service that will enable accountants to add value for decision makers over the coming decades. As information assurers, accountants can independently improve information *reliability* by auditing information that others prepare, and accountants also can independently improve information *relevance* by identifying, producing, and analyzing essential information through business advisory services (AICPA 1996). The *CPA Vision* statement emphasizes that enhancing information relevance "repositions the CPA higher on the value chain with clients and employers" and "moves the profession away from a backward looking, historical focus into a future or value-oriented mindset" (AICPA 1999b, 6).

CPA firms' relevance-improvement services include a variety of future-oriented activities, such as designing information systems for clients, advising them about particular transactions, and estimating cash flows from alternative future strategic actions (Böer 2000). The objectives and information needs for these engagements vary across clients, decisions, and contexts. Thus, an accountant typically must first identify the nature of the client's decision problem and create a problem representation[1] that reflects its key elements, then develop essential information—and do both without structured guidance.[2]

Because accountants face varying client objectives across relevance assurance engagements, they likely need knowledge gained through experience to help them select appropriate problem representations and develop relevant information. Thus, CPA firms, their clients, and accounting educators need to know the kind(s) of experience-based knowledge an accountant needs to provide high-quality information relevance advice for future-oriented decisions. For example, at the problem representation stage, the accountant may use knowledge gained through experience in a broad domain (such as accounting) to identify and represent a client's problem. Then, he or she may use a different type of knowledge gained through experience in a specialized domain (such as management accounting) to develop relevant information. Differences in objectives across relevance assurance engagements also make it difficult for CPA firms to develop standardized task presentation formats and work program templates to assist accountants. This means that a novel or inappropriate task presentation format may hamper even experienced accountants' access and use of their knowledge (Shanteau 1992).

We use an experiment to examine how experience—both in broad and specialized domains—and task presentation format affect accountants' problem representations and information development in a relevance assurance task. Our task requires participants to identify opportunity costs for timing of a client's disinvestment decision.[3] The task (implicitly) requires participants to estimate future cash flows for two alternative actions: disinvest now vs. disinvest later.

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[1] A problem representation is a mental picture or framework of the key components of a problem that specifies how to solve the problem (Anderson 2000, 264).

[2] In contrast, accountants conduct reliability-improvement services (such as historical financial statement audits) within a relatively constant, and relatively structured task assignment of obtaining sufficient competent evidence to form a GAAP-compliance opinion. Accountants obtain the well-defined knowledge needed for auditing through formal training and experience. Further, CPA firms have auditing standards and structured decision aids that help auditors construct appropriate and relatively invariant problem representations for all phases of audits.

We manipulate *task presentation format* between participants by providing either historical accounting earnings or historical cash flow information. To examine the effects of *experience* in broad vs. specialized domains, we recruit experienced accountants from both public and private (management) accounting. These participants are likely to possess similar amounts of experience in the broad domain of accounting, but different amounts of experience in the specialized domain of management accounting, specifically in developing information for future-oriented decisions. Consequently, they are likely to have different amounts of the types of knowledge required for the two stages of the task.

Stage one results suggest that task presentation format affects accountants' problem representations. Further, participants with more experience in either management or public accounting are more likely to select the appropriate future cash flow-based analysis as their problem representation. These findings suggest that broad domain experience can create one key element of knowledge required for performance in the task's problem representation stage—knowledge structures for recognizing problems that require analyses of cash flows.

Stage two results reveal that participants who have more management accounting experience develop more opportunity costs—if they choose a future cash flow analysis, but not if they choose a future earnings analysis. However, public accounting experience does not help participants to develop opportunity costs. We conclude that, because of their more extensive and varied specialized domain experience, only experienced management accountants develop a key type of knowledge that aids their performance beyond the problem representation stage—procedural rules for identifying relevant (future) opportunity costs.

The remainder of this paper contains four sections. Section II develops the hypotheses. Sections III and IV, respectively, present the research design and results. Section V discusses the results and possible implications.

II. BACKGROUND AND HYPOTHESES DEVELOPMENT

Relevance-Improvement Assurance Services

Accountants can apply relevance-improvement assurance services at any stage in the decision process (Elliott 1997, 1998; AICPA 1996). These stages include defining and representing the problem, determining information requirements, developing the information, and analyzing it to facilitate decision making.[4] In this paper, we examine accountants' relevance-improvement behavior at (1) the problem representation stage, and (2) the information development stage.

The specific task we examine (opportunity cost identification for a client's disinvestment timing decision) is inherently context-dependent and requires considerable knowledge to address. For example, it requires knowledge of the cost of capital, cost behavior, time value of money, typical business practices, and cash flow and opportunity cost concepts. Furthermore, there are no generally accepted templates or standards for developing a solution.

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[3] In this study, we use an instrument structured on the case in Vera-Muñoz (1998). While Vera-Muñoz (1998) examined the effect of declarative knowledge of financial accounting concepts on graduate business students' ability to identify opportunity costs, she could not address the effects of knowledge gained through accounting experience, because her participants had little accounting experience. Further, she did not manipulate presentation format or address the two-stage relation examined here.

[4] The AICPA's Special Committee on Assurance Services defines assurance services as "independent professional services that improve the quality of information or its context for decision makers" (AICPA 1996). Information quality refers to its reliability, relevance, and completeness for a particular decision. Information context includes all aspects of the decision setting and a decision model that defines relevant variables.

To develop a solution for such an unstructured task assignment, an accountant must draw on his or her knowledge related to the specific task. As indicated above, accountants must acquire this knowledge through experience in the general and specific domain of the task.

Domain Experience

Domain experience consists of encounters related to a particular discipline that provide the opportunity for acquiring knowledge (information stored in memory) about that discipline (Libby 1995). Experience can create three different types of knowledge:

1. *declarative knowledge* consisting of basic factual knowledge about the domain;
2. *knowledge structures* that organize basic factual knowledge around underlying principles or categories (e.g., accountants may have cash flow-based knowledge structures that reflect categories of financial items that affect future cash flows and costs [including opportunity costs], and historical accounting earnings-based knowledge structures that reflect categories of items that affect earnings such as sales transactions, purchase transactions, accruals, and allocations); and
3. *procedural knowledge* consisting of stored information about if-then rules that provide situation-specific solutions to problems (e.g., experienced management accountants might have the if-then rule: "if the decision involves a choice between paying cash now vs. later, then use the opportunity cost of capital [rather than historical interest expense rates] to compute the discounted value of future cash flows").

Amounts of broad domain experience (experience that is common across all specialized areas within a broad domain) and specialized domain experience (experience that is peculiar to a specialized area) differ across types of employment such as public and management accounting. Further, broad and specialized domain experience play different roles in the acquisition of the three types of knowledge described above. Declarative knowledge comes from the education and training that typically is part of broad domain experience. Consequently, we expect all our participants to have sufficient declarative knowledge to complete the experimental task. We also expect all of our participants to have developed multiple knowledge structures through experience in the broad domain of *accounting*. Finally, we expect that, because of management accounting's focus on the future, only our participants with experience in the specialized domain of *management accounting* have developed procedural knowledge for our task (Anderson 2000; Voss et al. 1983). Because all three types of knowledge gained through experience can play a role in performance of information relevance advisory services, we expect different effects of broad vs. specialized domain experience on performance of our experimental task.

Problem Representation Choice (Stage One)

The first stage of a relevance-improvement task is creating a problem representation, or framework or model, of a problem's structure that specifies how to solve it. Thus, a problem representation guides problem solution, and inappropriate problem representations can lead to poor solutions. Two factors expected to influence problem representations are domain experience (that leads to differences in knowledge structures) and task presentation format.

Experience

Experience in a domain leads to well-developed knowledge structures (e.g., Bonner et al. [1996] and Tubbs [1992] in auditing; Chi et al. [1981] in physics), and these experience-related knowledge structures have a major impact on problem representation quality. In a classic study, Chi et al. (1981) examine differences in problem representations between experienced and inexperienced physicists. Experienced physicists tend to represent problems based on underlying physics laws because their knowledge structures are organized around the laws of physics, while novices (who lack such knowledge structures) tend to focus on surface features such as whether a problem involves pulleys or inclined planes.[5] Researchers have not explored the relation between and among domain experience, knowledge structures, and problem representation for an accounting-related task.[6]

Task Presentation Format

Research in psychology and accounting finds that task presentation format affects the quality of judgment and decision making (e.g., Maines 1995; Hogarth 1987). A portion of format's effect on judgment derives from its effects on problem representation. Research on analogical transfer (Holyoak and Koh 1987; Spencer and Weisberg 1986; Stein et al. 1986) finds that task presentation format affects subjects' abilities to transfer their knowledge from a previously solved problem to create an appropriate representation for a new problem. In accounting, Hopkins (1996) finds that presentation format affects analysts' problem representations related to mandatorily redeemable preferred stock. Specifically, he finds that classification of the stock as debt or equity led to problem representations that used attributes of these financial instruments, whereas classification as neither debt nor equity (i.e., presented in the "mezzanine") led to problem representations that used individual attributes of the instrument.

Taken together, the lines of research on experience and presentation format suggest that more management accounting experience (related to the specialized domain of our task) leads to an appropriate (cash flow-based for our task) knowledge structure, and knowledge structure leads to an appropriate (cash flow-based for our task) problem representation for the task. This experience-based knowledge structure effect should occur only when accountants see the less appropriate (historical earnings-based) format. There should be no effect of management accounting experience in the historical cash flow format because this format should make cash flows salient, so that even accountants with low management accounting experience should choose the cash flow representation in this situation (e.g., Plous 1993).[7]

In contrast to the above predictions, there are at least two reasons why management accounting experience may not have a positive effect in the accounting earnings format. First, a recent study with experienced management accountants finds mixed support for a relation between experience and well-developed knowledge structures (Dearman and Shields 2001). If these results generalize, then our participants may not possess cash flow-based knowledge structures that enable them to create cash flow-based problem representations (in either presentation format condition). Second, performance observed in individuals with well-developed knowledge structures may depend on task characteristics such as presentation format as well as on their knowledge structures (Shanteau 1992). In particular, persons experienced with a particular presentation format may be affected adversely by deviations in format (e.g., Fischhoff 1982). Blessing and Ross (1996) find that experienced algebra problem-solvers often create an incorrect problem representation when faced with problems not presented in their "typical" format. This suggests that, although experienced management accountants may have appropriate knowledge structures, they may not access them when presented with an inappropriate format because they are accustomed to seeing an appropriate format.

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[5] Researchers in medicine (Lesgold et al. 1988), mathematics (Schoenfeld and Hermann 1982), and computer programming (Weiser and Shertz 1983) find similar results.

[6] Christ (1993) finds differences in audit-planning problem representations related to broad domain experience (tenure as an auditor). However, her measure of problem representations is very similar to a measure of knowledge structures.

[7] An incremental accounting earnings problem representation is appropriate in circumstances in which management's main objective is to maximize earnings. However, the (implicit) objective in our task is to maximize net cash flow.

Based on the notion that management accountants have extensive experience with cash flow-based problems and, consequently, very well-developed cash flow-based knowledge structures, we propose our first two hypotheses:

H1a: Management accounting experience will be positively related to the choice of a future cash flow problem representation when task information is presented in an historical earnings format.

H1b: Management accounting experience will not be related to the choice of a future cash flow problem representation when task information is presented in an historical cash flow format.

Research in accounting has not distinguished between specialized and broad domain experience when examining experience-related differences in knowledge structures. Specialized domain experience may not be necessary to create well-developed knowledge structures and high-quality problem representations (e.g., see Voss et al. [1983] for a study of political scientists). Research in accounting shows that, with experience in the broad domain of accounting, public accountants can develop and access multiple knowledge structures that organize accounting concepts around different features of those concepts (Bonner et al. 1996; Nelson et al. 1995; Frederick et al. 1994; Frederick 1991). Consequently, public accountants with broad domain experience may develop both accounting earnings-based and cash flow-based knowledge structures because they receive exposure to decision situations other than historical financial statement auditing (e.g., transaction-planning advice). Further, additional broad domain experience allows an individual greater access to knowledge structures in memory. Again, this advantage afforded by greater experience should occur only when accountants receive the historical earnings format because the cash flow format should make the appropriate representation salient.

In contrast to the above predictions, there are at least two reasons why public accounting experience may not have a positive effect on problem representation choice in the historical accounting earnings format. First, public accountants' experience consists predominantly of reporting historical outcomes; thus, they may be disadvantaged vis-à-vis management accountants in a future-oriented task because they possess less management accounting experience on average, and consequently, may not develop cash flow-based knowledge structures. Second, even if public accountants can develop the knowledge structures, set effect research (e.g., Anderson 2000) indicates that persons experienced in accessing certain knowledge structures tend to use the structures in creating problem representations, irrespective of problem content. For instance, Vera-Muñoz (1998) provides evidence that individuals with formal accounting training tend to access GAAP-based knowledge structures to construct a problem representation for an opportunity-cost identification problem similar to that used in this study. This suggests that public accountants would tend to access and use inappropriate knowledge structures for future-oriented decision problems.

Based on the previous accounting research demonstrating that public accountants can develop and access multiple knowledge structures, we propose the following hypotheses:

H2a: Public accounting experience will be positively related to the choice of a future cash flow problem representation when task information is presented in an historical earnings format.

H2b: Public accounting experience will not be related to the choice of a future cash flow problem representation when task information is presented in an historical cash flow format.

Relevance-Improvement Task Performance (Stage Two)

The second stage of the relevance-improvement task is development of relevant information. Two factors that are likely to affect relevant information development performance are problem representations and experience in a specialized domain (that leads to procedural knowledge).

Problem Representations

As mentioned earlier, inappropriate problem representations can lead to poor performance. Research in mathematics (Blessing and Ross 1996), medicine (Lesgold et al. 1988; Patel and Gröen 1986) and other areas (Anderson 2000) shows that problem representation quality affects identification of relevant information. The researchers' explanation is that problem representations naturally incorporate relevant categories of items and at least some relevant items within categories.

Experience

Some research indicates that individuals with greater domain experience have an advantage in problem solving beyond that conferred by well-developed knowledge structures. This advantage is due to *procedural knowledge*. Individuals acquire knowledge structures through experience in a *broad* domain. In contrast, individuals develop procedural knowledge through experience in a *specialized* domain (Anderson 2000). Specialized domain experience leads to development of knowledge of rules that are specific to particular tasks (Voss et al. 1983).

In accounting, two studies examine the relation between experience and procedural knowledge (Bonner and Walker 1994; Bonner et al. 1992), but do not examine the effects of specialized vs. broad domain experience on procedural knowledge. Also, several accounting studies examine whether there are differential performance effects of specialized vs. broad domain experience, but they do not relate experience to procedural knowledge (Solomon et al. 1999; Wright and Wright 1997; Johnson et al. 1991; Ashton 1991; Bonner and Lewis 1990).

Procedural knowledge may improve performance because task-specific rules can identify better solutions than can broad rules associated with problem representations. For example, Voss et al. (1983) find that political scientists without specialized experience related to the Soviet Union have similar problem representations to those with specialized experience, but their ultimate problem-solving performance is poorer because they lack Soviet Union-specific rules. As an example for opportunity-cost identification, the task-specific rule for discounting future cash flows at the cost of capital (rather than at historical interest rates) is superior to a generic rule for whether a financial item affects cash flow.

Furthermore, procedural knowledge seems to improve task performance only when an appropriate problem representation is in place. Problem representations incorporate knowledge structures, and persons with specialized experience have stronger mental links between knowledge structures and relevant procedural knowledge (Zeitz 1997; Bédard and Chi

1993). These links allow for easier access to information (Anderson 2000). No prior accounting studies have examined the joint effects of specialized domain experience and problem representations on task performance. However, based on the prior work in other areas, we expect that an appropriate problem representation will help accountants with specialized domain experience to access the relevant procedural knowledge, whereas an inappropriate problem representation will not.

Management accountants are likely to have specialized on-the-job experiences that will allow them to develop procedural knowledge for evaluating opportunity costs for alternative future-oriented decisions. This is because management accounting tasks are often focused on strategic and financial planning, budgeting, forecasting, and cash management alternatives that require use of future-oriented costs and benefits. This leads to an expectation that more management accounting experience will be related to identification of more opportunity costs, but only when accountants choose a future cash flow problem representation.

However, counter to the findings discussed above, some research suggests that there is no additional effect of procedural knowledge for individuals who choose appropriate problem representations (Vessey 1991; Lesgold 1984; Larkin 1983). These studies use more structured tasks, while the studies cited above use less structured tasks. Because our disinvestment-timing task is a less structured task, these latter results may not be applicable.[8] To test our predictions, we propose:

H3a: Management accounting experience will be positively related to the number of relevant opportunity costs identified when an accountant chooses a future cash flow problem representation.

H3b: Management accounting experience will not be related to the number of relevant opportunity costs identified when an accountant chooses a future accounting earnings problem representation.

In contrast, more public accounting experience is not likely to lead to development of more opportunity costs for either problem representation choice because public accountants' experience in the specialized domain of management accounting will not be sufficient to obtain relevant procedural knowledge. For example, many psychologists (e.g., Ericcson 1996) suggest that persons need thousands of hours of experience in a domain to obtain high levels of procedural knowledge. However, whether public accounting experience will have this effect is uncertain because previous studies (for a summary, see Bonner and Pennington 1991) have shown high-quality performance by public accountants in various tasks for which they clearly have not had thousands of hours of practice. Thus, the effect of public accounting experience is uncertain, but based on the important relation of specialized domain experience to procedural knowledge we propose:

H4a: Public accounting experience will not be related to the number of relevant opportunity costs identified when an accountant chooses a future cash flow problem representation.

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[8] Additionally, Dearman and Shields' (2001) results regarding experience and knowledge structures may suggest that it is difficult to acquire procedural knowledge through specialized management accounting experience due to lack of informative feedback.

and the going rate of lease renewal. Furthermore, participants received the current book value of five-year bonds for possible calculation of accounting gain on early retirement of the bonds, as well as parallel information about their current market value for calculating the forgone return on the bonds' early retirement at their market value.[10]

Participants and Administration

We solicited participants with at least two years of accounting experience, some of whom were principally public accountants and some of whom were principally management accountants in private industry. We measure management accounting (MA) experience and public accounting (PA) experience using the square root of months of MA experience and the square root of months of PA experience, respectively.[11]

To ensure adequate variation in both MA and PA, we recruited participants from several groups of accounting professionals. We mailed a packet containing a cover letter plus the case materials for Part One to 350 CPAs randomly selected from the AICPA membership rosters from three Midwestern states, and to 50 Executive M.B.A. (E.M.B.A.) alumni in accounting positions randomly selected from the University of Notre Dame's E.M.B.A. alumni directory. The post office returned eight packets due to insufficient or unknown addresses. Forty-three CPAs and ten E.M.B.A. alumni completed the study, for response rates of 13 percent and 20 percent, respectively.[12] In addition to these mailings, 13 accountants currently enrolled in the university's E.M.B.A., and 44 audit associates from a large national CPA firm voluntarily completed the study (three additional associates failed to provide supporting computations and were excluded). The audit associates participated at their firm's national education center in four different sessions over a three-month period.[13,14] The others completed the experiment at remote locations, and faxed the response memorandum and supporting calculations for Part One to the experimenters. Immediately upon receiving the completed Part One, one of the authors faxed or e-mailed Part Two to the participant, who in turn either faxed or e-mailed the completed Part Two.

Table 1 shows that usable response rates were about equal across the two task presentation formats, and that our participant solicitation techniques were successful in that the

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[10] There were some inevitable differences between presentation formats, but these differences had to do with irrelevant items (e.g., depreciation and bond premium amortization), which we exclude from our analyses herein because they are not opportunity costs. Furthermore, we regressed the total number of irrelevant items included in the participants' analyses on the independent variables in our stage-two regression model in Section IV. Management accounting experience and public accounting experience had no relation to the number of irrelevant items included ($p = 0.951$ and $p = 0.419$, respectively).

[11] We use square root measures to reflect likely diminishing incremental effects of experience. In contrast to Mikhail et al. (1997), we use the square root form rather than power or logarithmic transformation because some managers have zero experience in management accounting or public accounting. We obtain similar results using raw months of MA and PA.

[12] We anticipated a low response rate due to the substantial task demands. To provide participation incentives, we entered the participants in a lottery for 24 football tickets for the University of Notre Dame's home games, plus a grand prize consisting of a cash award of $450 to cover air fare for two persons. We had very limited data to examine differences between respondents and nonrespondents because of information limitations placed upon us by the AICPA and E.M.B.A. program. We compared respondents and nonrespondents, therefore, on only employer type and gender. There were no differences between these groups on firm, but respondents included a higher percentage of males than did nonrespondents. Adding gender to the analyses does not qualitatively change the results.

[13] One of the experimenters was present at all of the sessions. Subjects sitting contiguously worked on different experimental conditions. The experimenter asked the auditors and their instructors to refrain from discussing the study with their co-workers once back at their local offices.

[14] The audit associates completed Part One in 46 minutes, on average, and the CPAs and E.M.B.A.s completed Part One in 49 minutes, on average. These means are not statistically different ($p = 0.371$), suggesting that differences in experimental administration methods (mailing vs. on-site) did not affect the time required to complete the experimental task.

TABLE 1
Descriptive Statistics: Independent Variables, Covariate, and Dependent Variables (n = 110)

Variables	*Means or Frequencies*	
Task Presentation Format[a] (FORMAT):		
Historical accounting earnings	n = 53 (48.18%)	
Historical cash flow	n = 57 (51.82%)	
	Management	*Public*
Accounting Experience in Months		
AICPA members	104	101
E.M.B.A.s	159	18
Audit Associates	10	47
Mean	78	62
Opportunity Cost Knowledge Score[b] (TESTSCORE)	5.11 (s.d. = 1.33)	
Analysis Method Choice[c] (CHOICE):		
Future accounting earnings	n = 34 (31%)	
Future cash flow	n = 76 (69%)	
Number of Opportunity Costs Identified (OPPCOSTS):[d]	2.65 (s.d. = 1.79)	

[a] We randomly assigned participants to one of two task presentation formats, historical accounting earnings-based or historical cash flow-based.

[b] Mean opportunity cost knowledge score, measured by participants' numbers of correct responses on a six-question opportunity cost knowledge test.

[c] The participants' choice of information analysis method, classified as either future accounting earnings or future cash flow.

[d] Mean number of opportunity costs participants identified (maximum possible is six, minimum is zero).

110 participants with usable responses varied substantially on MA and PA. Participants holding management accounting positions described their titles as corporate controllers, senior corporate accountants, chief financial officers, and chief operating officers. Their job responsibilities included strategic and financial planning, budgeting, forecasting, cash management and control, and financial analysis. Participants with public accounting positions described their titles as audit associates, managers, or partners in national, regional, or local CPA firms, and their job responsibilities included all aspects of historical financial statement audits, consulting, and, in a few cases, tax-related tasks. The differences in typical responsibilities imply experiences that would likely produce substantially different types of knowledge.

Variables

We use the explanations and supporting calculations provided by each participant's response memorandum to classify their analysis method (denoted CHOICE) as future cash flow-based if the participant prepared a future cash flow-based analysis, or future earnings-based if the participant prepared a future earnings-based analysis. We determine the number of opportunity costs included in a participant's analysis (denoted OPPCOSTS) by examining

TABLE 2
Cross-Classifications Tables
For Number of Opportunity Costs Participants Identified (OPPCOSTS)[a]

Panel A: Task Presentation Format (FORMAT) and Analysis Method Choice (CHOICE)

	CHOICE		
FORMAT	*Future Accounting Earnings*	*Future Cash Flow*	*Total*
Historical Accounting Earnings	1.41 (27)	3.12 (26)	2.25 (53)
Historical Cash Flow	1.43 (7)	3.26 (50)	3.04 (57)
Total	1.41 (34)	3.21 (76)	2.65 (110)

Panel B: Public Accounting Experience, Management Accounting Experience, and CHOICE

	CHOICE = Future Accounting Earnings			*CHOICE = Future Cash Flow*		
	Public Accounting Experience			*Public Accounting Experience*		
Management Accounting Experience	*<60 mon.*	*≥60 mon.*	*Total*	*<60 mon.*	*≥60 mon.*	*Total*
<60 mon.	1.47 (19 [17, 2])	1.60 (5 [3, 2])	1.50 (24 [20, 4])	2.71 (17 [5, 12])	2.90 (21 [9, 12])	2.82 (38 [14, 24])
≥60 mon.	1.20 (10 [7, 3])	—	1.20 (10 [7, 3])	3.87 (23 [9, 14])	3.20 (15 [3, 12])	3.61 (38 [12, 26])
Total	1.38 (29 [24, 5])	1.60 (5 [3, 2])	1.41 (34 [27, 7])	3.37 (40 [14, 26])	3.03 (36 [12, 24])	3.21 (76 [26, 50])

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[a] The number of accountants in each classification is shown in parentheses, and in Panel B the numbers of accountants receiving historical accounting earnings and historical cash flow task presentation formats, respectively, are in brackets ([,]).

problem representation performance. Hypotheses 1b and 2b predict that, when the task is presented in an historical cash flow format, more management accounting or more public accounting experience will have no effect on problem representation performance. To test these hypotheses, related to stage one of the task, we estimate the following logistic regression model:

$$\begin{aligned} \text{CHOICE}_i = {} & \beta_0 + \beta_1\text{FORMAT}_i + \beta_2\text{MA}_i + \beta_3\text{PA}_i + \beta_4(\text{MA}_i \times \text{FORMAT}_i) \\ & + \beta_5(\text{PA}_i \times \text{FORMAT}_i) + \beta_6\text{TESTSCORE}_i + \varepsilon_i \qquad (1) \end{aligned}$$

where CHOICE, our proxy for problem representation, is coded 1 = future cash flow analysis and 0 = future accounting earnings analysis, FORMAT is coded 1 = historical cash flow-based format and 0 = historical accounting earnings-based format, MA and PA denote the square roots of the numbers of months of management accounting experience and public accounting experience, respectively, and TESTSCORE is the participants' declarative opportunity cost knowledge score.

In equation (1) the intercepts and slopes are allowed to vary between MA and PA, and by FORMAT. The intercept β_0 applies to participants in the historical accounting earnings format, and the incremental intercept β_1 is the difference due to the historical cash flow format. The slope coefficients β_2 and β_3 are the effects of MA and PA, respectively, for the historical accounting earnings format, as predicted by H1a and H2a. β_4 and β_5 measure the *incremental* (relative to β_2 and β_3) effects, respectively, of MA and PA for the historical cash flow format, while $\beta_2 + \beta_4$ and $\beta_3 + \beta_5$ measure the ***aggregate*** effects of MA and PA, respectively, for the historical cash flow format, thus testing H1b and H2b. The coefficient β_6 represents the effect of participants' opportunity cost knowledge.

Table 3, Panel A presents a summary of logistic regression results estimating equation (1), and Panel B shows our predictions to test the stage one hypotheses.[19] Panel A shows that task presentation format (coefficient β_1) significantly affects choice of analysis method, with an historical cash flow presentation format increasing the likelihood of choosing a future cash flow analysis method ($p < 0.01$, one-tailed). Also, the coefficient for TESTSCORE (β_6) is significantly positive ($p < 0.01$, one-tailed), indicating that basic opportunity cost knowledge helps participants choose the proper analysis.

Panel B of Table 3 shows our predictions and the related regression estimates. When participants receive task information in an historical earnings format, MA (coefficient β_2) increases the likelihood he or she will choose a future cash flow analysis ($p < 0.01$, one-tailed), supporting H1a. When participants receive task information in an historical earnings format, PA (β_3) also increases the likelihood of a future cash flow analysis choice ($p < 0.01$, one-tailed), supporting H2a. In contrast, the aggregate effects of MA ($\beta_2 + \beta_4$) and PA ($\beta_3 + \beta_5$) for those receiving the historical cash flow format are not significantly different from zero at conventional levels, consistent with H1b and H2b, respectively.[20]

Taken together, the Table 3 results indicate that initial task presentation format is an important determinant of analysis method choice, *per se*. Also, consistent with our predictions, when the task presentation is in the less appropriate format (historical accounting earnings-based), accountants with more MA or PA are more likely to select the appropriate analysis method. However, when the task presentation is in the more appropriate format, neither more MA nor more PA increases their tendency to choose the appropriate analysis method.

Hypothesis 3a predicts better average information development performance for participants with more MA when they choose a future cash flow analysis. Specifically, H3a predicts that MA will have a positive effect on OPPCOSTS when participants choose a future cash flow analysis. In contrast, H4a predicts that PA will not be related to OPPCOSTS performance when participants choose a future cash flow analysis. Further, H3b and H4b predict no relation between MA and PA, respectively, and OPPCOSTS when participants choose the inappropriate future accounting earnings analysis.

To test these hypotheses related to stage two of the task, we estimate the following model:

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[19] Since CHOICE is a dichotomous measure, the model above does not meet the assumptions of OLS regression. We estimated the model using both logistic and OLS regressions with the same qualitative results. Only the logistic regression results are reported.

[20] This is because the incremental effects of MA (β_4) and PA (β_5) in the historical cash flow format are significantly negative ($p < 0.05$).

TABLE 3
Logistic Regression Model, Predictions, and Results: Stage One—Analysis Method Choice (n = 110)

$$CHOICE_i^a = \beta_0 + \beta_1 FORMAT_i + \beta_2 MA_i + \beta_3 PA_i + \beta_4 (MA_i \times FORMAT_i) + \beta_5 (PA_i \times FORMAT_i) + \beta_6 TESTSCORE_i + \varepsilon_i$$

Panel A: Logistic Regression Results

	β_0	β_1	β_2	β_3	β_4	β_5	β_6
Coefficient estimate	−10.175	6.913	0.306	0.727	−0.218	−0.654	0.848
Wald statistic	16.473***	8.982***	10.191***	10.280***	2.825**	5.075**	12.192***

Model Summary Statistics	
−2Log Likelihood	79.928
Chi-Square for Model (6 df)	63.114
p-value	0.000
Percentage Correctly Classified	86.4%

Panel B: Predictions and Results for Effects of Accounting Experience and Task Presentation Format

Test	*Format-Experience Combination*[b]	*Coefficient*	*Expected Sign*	*Coefficient Estimate*	*Wald Statistic*
H1a	HAE-MA	β_2	+	0.306	10.191***
H1b	HCF-MA	$\beta_2 + \beta_4$	0	0.088	0.952
H2a	HAE-PA	β_3	+	0.727	10.280***
H2b	HCF-PA	$\beta_3 + \beta_5$	0	0.073	0.208

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*, **, and *** indicate significance at $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively (one-tailed).

[a] $CHOICE_i$ = 1 if participant i's choice of analysis method was future cash flow, and 0 if choice was future accounting earnings;
$FORMAT_i$ = 1 if participant i received the historical cash flow format, and 0 for the historical accounting earnings format;
MA_i = square root of participant i's months of management accounting experience;
PA_i = square root of participant i's months of public accounting experience;
$TESTSCORE_i$ = participant i's score on the opportunity cost knowledge test; and
ε_i = error term.

[b] HAE, HCF = historical accounting earnings, historical cash flow, respectively.

$$OPPCOSTS_i = \beta_0 + \beta_1 AECHOICE_i + \beta_2 MA_i + \beta_3 PA_i + \beta_4 (MA_i \times AECHOICE_i) + \beta_5 (PA_i \times AECHOICE_i) + \beta_6 TESTSCORE_i + \varepsilon_i \qquad (2)$$

where OPPCOSTS denotes the number of relevant opportunity costs identified (OPPCOSTS

$= 0,\ldots,6$), AECHOICE is coded $1 =$ future accounting earnings analysis choice and $0 =$ future cash flow analysis choice, and other variables are as defined above.[21]

In equation (2) the intercepts and slopes are allowed to vary between MA and PA, and by AECHOICE. The intercept β_0 applies to participants in the future cash flow analysis, and the incremental intercept β_1 is the difference due to the future accounting earnings analysis. The slope coefficients β_2 and β_3 are the effects of MA and PA, respectively, for those choosing a future cash flow analysis as predicted by H3a and H4a. In contrast, β_4 and β_5 measure the *incremental* (relative to β_2 and β_3) effects, respectively, of MA and PA for the future accounting earnings analysis, while $\beta_2 + \beta_4$ and $\beta_3 + \beta_5$ measure the *aggregate* effects of MA and PA, respectively, for those choosing a future accounting earnings analysis, thus testing H3b and H4b. The coefficient β_6 is as defined above for equation (1).

Table 4, Panel A presents a summary of regression estimates of equation (2) and Panel B shows our predictions to test the stage two hypotheses. Panel B shows a significant and positive effect of MA given the choice of a future cash flow analysis: MA (β_2) increases OPPCOSTS when accountants choose a future cash flow analysis ($p < 0.01$, one-tailed), supporting H3a. In contrast, PA (β_3) is not related to OPPCOSTS when accountants choose a future cash flow analysis ($p > 0.34$, one-tailed), consistent with H4a.[22] The aggregate effects of MA ($\beta_2 + \beta_4$) and PA ($\beta_3 + \beta_5$) in the future accounting earnings analysis are not significantly different from zero at conventional levels, consistent with H3b and H4b.[23]

Overall, the effect of management accounting experience on performance is beneficial when participants choose the proper analysis, and has no effect otherwise. Taken together, these results are consistent with our prediction that only management accounting experience significantly improves relevant information identification performance beyond the problem representation stage, and only when participants choose the more appropriate problem representation.

V. DISCUSSION AND IMPLICATIONS

This paper uses an experiment with a two-stage task to examine the separate and combined effects of two factors that may affect accountants' performance in relevance-improvement assurance tasks: experience (both in broad and specialized domains) and task presentation format. In this final section, we briefly discuss our findings, some implications of the findings for relevance assurance practice and research, and some limitations of the study.

At the problem representation stage of our task, we find that when the participants receive the less appropriate historical accounting earnings-based task presentation format, more experience in either public accounting or management accounting helps them choose the more appropriate future cash flow analysis method. This suggests that broad accounting experience obtained in either public or management accounting positions allows accountants to develop and access an appropriate knowledge structure (consistent with Frederick [1991] and Frederick et al. [1994], but in contrast to Nelson et al. [1995]). We also find that presentation of the task in the more appropriate cash flow format allows inexperienced

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[21] Note that the coding of AECHOICE is the opposite of CHOICE (i.e., AECHOICE $= 1 -$ CHOICE) to facilitate notation for testing H3a and H4a.

[22] We also estimated regressions for the CHOICE model (stage one) and OPPCOSTS model (stage two) using a dichotomous measure of MA and PA with accountants having less than 36, 60, or 120 months classified as having "low" experience. All results were qualitatively similar to those reported using the square roots of months of experience.

[23] This is because the incremental effect of MA in the future accounting earnings analysis (β_4) is significantly negative ($p < 0.05$, one-tailed), and the incremental effect of PA in the future accounting earnings analysis (β_5) is not significant at conventional levels.

TABLE 4
OLS Regression Model, Predictions, and Results: Stage Two—Number of Opportunity Costs Identified
(n = 110)

$$OPPCOSTS_i^a = \beta_0 + \beta_1 AECHOICE_i + \beta_2 MA_i + \beta_3 PA_i + \beta_4(MA_i \times AECHOICE_i) + \beta_5(PA_i \times AECHOICE_i) + \beta_6 TESTSCORE_i + \varepsilon_i$$

Panel A: OLS Regression Results

	β_0	β_1	β_2	β_3	β_4	β_5	β_6
Coefficient estimate	2.415	−0.336	0.081	−0.019	−0.155	−0.102	0.065
t-statistic	2.835***	−0.319	2.609***	−0.397	−2.042**	−0.767	0.523

Model Summary Statistics	
F-statistic	7.123 (p = 0.000)
R^2	0.293
R^2-adjusted	0.252
Std. Error	1.552

Panel B: Predictions and Results for Effects of Accounting Experience and Choice of Information Analysis Method

Test	*Choice-Experience Combination*[b]	*Coefficient*	*Expected Sign*	*Coefficient Estimate*	*t-Statistic*
H3a	FCF-MA	β_2	+	0.081	2.609***
H3b	FAE-MA	$\beta_2 + \beta_4$	0	−0.074	−1.071
H4a	FCF-PA	β_3	0	−0.019	−0.397
H4b	FAE-PA	$\beta_3 + \beta_5$	0	−0.121	−0.980

---

*, **, and *** indicate significance at $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively (one-tailed).

[a] OPPCOSTS$_i$ = the number of opportunity costs identified by participant i;

AECHOICE$_i$ = 1 if participant i's choice of analysis method was future accounting earnings, and 0 if choice was future cash flow.

[b] FAE, FCF = future accounting earnings analysis, future cash flow analysis, respectively.

See Table 3 for other variable definitions.

accountants to choose the future cash flow analysis method, suggesting that the initial task presentation can substitute for some aspects of broad domain experience.

At the information development stage, we find that experience in the specialized domain of management accounting can help information development task performance, but only when participants choose the more appropriate future cash flow analysis method. To our knowledge, no prior study in accounting has demonstrated that choice of analysis method (a proxy for problem representation) and specialized domain experience (which we assume leads to procedural knowledge for our task) jointly affect accountants' ability to develop relevant information. The finding suggests that accountants need specific domain experience for maximum performance, but they must choose the appropriate analysis method to fully realize the benefit of their domain experience.

One broad question for practice and future research that arises from our findings is: How can firms facilitate accountants' choice of appropriate analysis methods across various types of information relevance-improvement engagements? One approach might be to develop guidance based on concepts from decision analysis (Elliott 2000; Elliott and Pallais 1997). For example, at the start of an assignment accountants could explicitly identify the client's objective such as "maximize cash flow," "maximize earnings," or "maximize stock value." Also, it may be possible to prepare broad guidance on typical considerations for customizing information from multiple disciplines (AICPA 1996). For example, a template listing information categories typically relevant when considering a possible foreign venture could facilitate information completeness advice. Such a template might include historical data and projections about external competitive, product, and labor markets, internal financial and nonfinancial operating measures, and foreign country laws and regulations, taxes, and cultural considerations that could affect operations and controls.

A second broad question that arises is: How can accountants who lack the procedural knowledge to perform a particular relevance-improvement task obtain that knowledge? One possible answer is development of a central advisory "experience base" that combines the procedural knowledge (situation-specific rules) of multiple individuals, each of whom have had only a few experiences with a specific task, as some CPA firms currently provide to enhance individual auditors' industry knowledge for audit engagements. Whether this particular approach will be effective is an open question, however, because prior research indicates that procedural knowledge can be gained only through personal experience.

A third broad question arises because our experiment examines a single information relevance-improvement task that uses several measurement concepts central to management accounting experience and future cash flow analysis in a single decision context. The question is: Do our conclusions hold for other types of experience, analyses, and contexts? The generality of our findings is an important question because CPA firms offer a wide variety of information relevance-improvement services that integrate a broad range of financial, strategic, and operating measures through new information technologies. Various types of management accounting, taxation, information technology, and systems experiences may help accountants better perform some types of information-relevance tasks, whereas financial accounting experience may help accountants perform other kinds of tasks. For example, future research could examine whether a public accountant's experience auditing clients' risk disclosures for derivatives may facilitate risk-based advisory services that assess uncertainties about future outcomes. Similarly, future research could address whether audit experience evaluating client strategies (Bell et al. 1997) and information technology applications (Public Oversight Board 2000) may help accountants better perform strategy- and technology-related relevance improvement assignments. Finally, our experiment uses traditional financial measures. Our results may not hold for relevance assurance services that integrate nonfinancial leading indicators of performance or critical success factors, such as customer satisfaction, employee training and satisfaction, and product service and quality (Kaplan and Norton 1996). Future research could examine whether industry experience and knowledge of corporate strategy may help accountants perform these services (see Solomon et al. [1999] for an example in auditing).

Furthermore, future research could develop and test theories about what types of accounting knowledge facilitate particular types of relevance assurance services, and how accountants (or a CPA firm) can best acquire that knowledge. From a practitioner's perspective, research may establish the comparative advantages of accountants in performing these services. Also, from an educator's perspective, research could assess how nonaccountants (e.g., managers, consultants, and information technology professionals) can best acquire and use knowledge of accounting concepts such as cost behavior, cash flow, accounting allocations and estimates, regulations, and taxes to improve the quality of information or its context for decision making.

Finally, this study has limitations due to measurement. Specifically, we propose that management accounting experience leads to both well-developed knowledge structures and procedural knowledge, and that public accounting experience leads only to well-developed knowledge structures. We do not measure these important mediating variables (knowledge structures and procedural knowledge). The value of our professional participants' time prohibited us from creating a lengthy instrument that would directly measure both these mediators. In addition, procedural knowledge is difficult to measure directly, and consequently, typically is inferred through task performance (Anderson 2000; Herz and Schultz 1999). Because our dependent variable was task performance, however, we could not use this as a measure for procedural knowledge. Future research could develop better measures of procedural knowledge.

Another limitation of not measuring knowledge structure and procedural knowledge as mediating variables is that experience also can be associated with other differences in individuals. For example, over time, firms may select individuals for promotion based on factors such as ability. Additional research that measures the mediating variables can test our assumptions about the mechanisms by which broad and specialized domain experience affect performance in information relevance assurance tasks, and assess whether there are additional important determinants of performance in these tasks, such as various types of abilities.

APPENDIX A

Experimental Instrument: Historical Earnings-Based Task Presentation Format

Andrew, Henderson & Company
Financial Planning Consultants
12-23-X7

STAFF MEMORANDUM
SUBJECT: Operations of Harper & Baker's Sacramento Store

Harper & Baker's Sacramento Store
Statement of Income
For the year ended December 31, 19X7[1]

Revenues:	
Sales	$1,920,000
Interest (*Note 1*)	5,400
Total Revenues	$1,925,400
Costs and Expenses:	
Cost of Sales (*Note 2*)	576,000
Operating, selling, general and administrative expenses (*Note 2*)	1,020,000
Interest Costs:	
Bonds (*Note 3*)	12,000
Total Costs and Expenses	$1,608,000
Net Income	$317,400

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[1] *December figures are estimates.*

(*Continued on next page*)

Note 1: Interest Revenue

In 19X7 all the H&B retail stores initiated a company policy of investing their excess cash in a common fund managed by the parent company. Each store received a pro-rata share of its investment in the common fund. The Sacramento store earned $5,400 this year on an annual investment of S30,000. This investment and return are expected to remain constant over the next two years.

Note 2: Cost of Sales and OSG&A Expenses

The cost of goods sold represents approximately 30% of sales. Operating, selling, general and administrative expenses (OSG&A) of $1,020,000 in 19X7 include advertising ($120,000), depreciation ($36,000), operating lease ($96,000), bond premium amortization ($1,000), and other fixed expenses ($767,000).

Note 3: Interest on Bonds

As part of a city government's business incentive program, H&B's Sacramento store issued $100,000 of five-year bonds three years ago. The annual interest on the bonds is $12,000. The current book value of the bonds is $101,000. Because the interest rates have risen, H&B can purchase the bonds in the market at $94,900. If Evans chooses to vacate the retail space now, then H&B would have to retire the bonds now, thus yielding an accounting gain of $6,100. If, on the other hand, Evans chooses to vacate the retail space one year from now, then H&B would liquidate the bonds at their maturity date.

Prospects for the Next Two Years (19X8 and 19X9)

Disposition of Fixed Assets

The Sacramento store's fixed assets, which include display fixtures and equipment, were acquired 7 years ago at $360,000. Depreciation for financial statement purposes is provided on the straight-line method over an estimated average useful life of 10 years, with zero salvage value.

Evans has decided to sell some of the store's display fixtures and equipment at the time that the store's current retail space is vacated. Evans estimates that the assets can be sold for about $15,000 either now or one year from now. The net book value of those assets now is $30,000, and will be $20,000 one year from now; thus, the assets' disposal would yield an accounting loss of either $15,000 or $5,000, respectively.

Lease Contract and Compensation Award

Four years ago H&B signed a 10-year transferable, noncancelable operating lease for the 4,000 square-foot retail location of the Sacramento store, at $96,000 per year. Due to a recent earthquake, there is shortage of retail space in Sacramento; thus, lease renewals are currently running at about $144,000 per year for a 4,000 square-foot location. If Evans chooses to vacate the retail space now, she could sublease it to a third party for the next two years at the market rate. Alternatively, if Evans chooses to vacate the retail space one year from now, then she could sublease it to a third party for just 19X9 for $80,000. Since RPM will be unable to fulfill its lease contract with H&B, it has offered to pay H&B a compensation award of $300,000 at the time that Evans vacates the retail space.

Projected Annual Operations

For either option, average sales are expected to remain essentially unchanged for the next two years, and the cost of goods sold is expected to remain at 30% of sales. Regarding OSG&A expenses, advertising is expected to increase due to a grand-opening campaign scheduled to be launched the year of the relocation. The following table summarizes projected annual sales and OSG&A expenses for the Sacramento store for 19X8 and 19X9:

Excerpts from Projected Income Statement for 19X8 and 19X9

	Vacates Sacramento Retail Space			
	Now		One Year from Now	
	19X8	19X9	19X8	19X9
Sales	$1,920,000	$1,920,000	$1,920,000	$1,920,000
OSG&A expenses:				
Advertising	300,000	120,000	120,000	300,000
Depreciation	26,000	26,000	36,000	26,000
Operating lease	96,000	96,000	96,000	96,000
Bond premium amortization	1,000	1,000	1,000	1,000
Other fixed expenses	767,000	767,000	767,000	767,000
Total OSG&A expenses	$1,190,000	$1,010,000	$1,020,000	$1,190,000

APPENDIX B
Experimental Instrument: Historical Cash Flow-Based Task Presentation Format

Andrew, Henderson & Company
Financial Planning Consultants
12-23-X7

STAFF MEMORANDUM
SUBJECT: Operations of Harper & Baker's Sacramento Store

Harper & Baker's Sacramento Store
Statement of Operating Cash Flows
December 31, 19X7[1]

Cash flows from operating activities:	
Cash collections:	
From customers	$1,920,000
From interest (*Note 1*)	5,400
Total cash collections	$1,925,400
Cash payments:	
To suppliers and vendors (*Note* 2)	576,000
For operating, selling, general and administrative costs (*Note* 2)	983,000
For interest on bonds (*Note 3*)	12,000
Total cash payments	$1,571,000
Net cash provided by operating activities	$354,400

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[1] *December figures are estimates.*

Note 1: Cash Collections from Interest

In 19X7 all the H&B retail stores initiated a company policy of investing their excess cash in a common fund managed by the parent company. Each store received a pro-rata share of its investment in the common fund. The Sacramento store earned $5,400 this year on an annual investment of $30,000. This investment and return are expected to remain constant over the next two years.

Note 2: Cash Flows Used in Current Business Operations

As a standard practice, the Sacramento store pays in cash to their suppliers and vendors their inventory purchases and operating costs. The cost of sales represents approximately 30% of sales. Cash payments for operating, selling, general and administrative costs of $983,000 in 19X7 include advertising ($120,000), operating lease ($96,000), and other fixed costs ($767,000).

Note 3: Cash Payments for Interest on Bonds

As part of a city government's business incentive program, H&B's Sacramento store issued $100,000 of five-year bonds three years ago. The annual interest on the bonds is $12,000. The current book value of the bonds is $101,000. Because the interest rates have risen, H&B can purchase the bonds in the market at $94,900. If Evans chooses to vacate the retail space now, then H&B would have to retire the bonds now, thus yielding an accounting gain of $6,100. If, on the other hand, Evans chooses to vacate the retail space one year from now, then H&B would liquidate the bonds at their maturity date.

Prospects for the Next Two Years (19X8 and 19X9)

Disposition of Fixed Assets

The Sacramento store's fixed assets, which include display fixtures and equipment, were acquired 7 years ago at $360,000. Depreciation for financial statement purposes is provided on the straight-line method over an estimated average useful life of 10 years, with zero salvage value.

Evans has decided to sell some of the store's display fixtures and equipment at the time that the store's current retail space is vacated. Evans estimates that the assets can be sold for about $15,000 either now or one year from now. The net book value of those assets now is $30,000, and will be $20,000 one year from now; thus, the assets' disposal would yield an accounting loss of either $15,000 or $5,000, respectively.

Lease Contract and Compensation Award

Four years ago H&B signed a 10-year transferable, noncancelable operating lease for the 4,000 square-foot retail location of the Sacramento store, at $96,000 per year. Due to a recent earthquake, there is shortage of retail space in Sacramento; thus, lease renewals are currently running at about $144,000 per year for a 4,000 square-foot location. If Evans chooses to vacate the retail space now, she could sublease it to a third party for the next two years at the market rate. Alternatively, if Evans chooses to vacate the retail space one year from now, then she could sublease it to a third party for just 19X9 for $80,000. Since RPM will be unable to fulfill its lease contract with H&B, it has offered to pay H&B a compensation award of $300,000 at the time that Evans vacates the retail space.

Projected Annual Operating Cash Flows

For either option, average sales are expected to remain essentially unchanged for the next two years, and the cost of goods sold is expected to remain at 30% of sales. Regarding OSG&A costs, advertising is expected to increase due to a grand-opening campaign scheduled to be launched the year of the relocation. The following table summarizes projected annual cash flows from operating activities for the Sacramento store for 19X8 and 19X9:

Excerpts from Projected Statement of Operating Cash Flows for 19X8 and 19X9

	Vacates Sacramento Retail Space			
	Now		One Year from Now	
	19X8	19X9	19X8	19X9
Cash flows from operating activities:				
Cash collections:				
From customers	$1,920,000	$1,920,000	$1,920,000	$1,920,000
Cash payments:				
Advertising	300,000	120,000	120,000	300,000
Operating lease	96,000	96,000	96,000	96,000
Other fixed costs	767,000	767,000	767,000	767,000
Total cash payments	$1,163,000	$983,000	$983,000	$1,163,000

APPENDIX C

Suggested Analysis for Timing of Disinvestment Decision Using Incremental Future Cash Flows (in $000s)

Option A: Client vacates now (12-31-X7), and moves the store to its new location on 1-1-X8

	12-31-X7	12-31-X8	12-31-X9
Incremental advertising outlays		($180)	
Bond retirement	($95)		
Proceeds from assets' disposal	15		
Sublease revenue		48	$48
Compensation award	300		
Total before-tax cash flows	$220	($132)	$48
Net Present Value (NPV) as of 12-31-X7, at 18% cost of capital	$143		

Option B: Client vacates one year from now (12-31-X8), and moves the store to its new location on 1-1-X9

	12-31-X7	12-31-X8	12-31-X9
Incremental advertising outlays			($180)
Bond retirement			(100)
Proceeds from assets' disposal		$15	
Forgone sublease revenue			(16)
Compensation award		300	
Total before-tax cash flows	$0	$315	($296)
Net Present Value (NPV) as of 12-31-X7, at 18% cost of capital	$54		

Recommendation: The client should vacate now (12-31-X7), and move the store to its new location on 1-1-X8.

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Securities Price Consequences of the Private Securities Litigation Reform Act of 1995 and Related Events

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ABSTRACT: The Private Securities Litigation Reform Act (PSLRA) increases restrictions on private litigation for securities fraud. We examine stock price reactions on legislative-event-related days of firms in four high-litigation-risk industries. Two other studies on this issue, Spiess and Tkac (1997) (ST) and Johnson et al. (2000) (JKN), conclude that shareholders considered PSLRA beneficial. While we find largely similar daily abnormal returns for event-related days that they examine, we present evidence that the timing of multiple confounding events makes the interpretation of these daily returns ambiguous. Results from additional analyses beyond those conducted by ST and JKN (market price reversal tests, analysis of additional legislative-event-related days, cumulative abnormal returns over the legislative period, and analysis of other events affecting investors' ability to bring securities-related lawsuits), are largely inconsistent with their interpretation, suggesting instead that shareholders in the four high-litigation-risk industries react *negatively* on average to PSLRA's restrictions on their ability to bring securities-related lawsuits.

Keywords: *Private Securities Litigation Reform Act of 1995; Proposition 211 on the 1996 California state ballot; aiding and abetting of securities fraud; shareholders.*

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I. INTRODUCTION

In response to a perceived litigation explosion, Congress enacted the Private Securities Litigation Reform Act of 1995 (PSLRA), which increases restrictions on private litigants' ability to sue for investment losses from securities fraud. This legislation stimulated heated debate on whether it would benefit or harm shareholders (Avery 1996). The cost of defending a securities lawsuit falls directly on the company, and indirectly on its shareholders (Kraakman et al. 1994). These investors incur unnecessary costs when plaintiffs or their lawyers abuse the system by filing frivolous lawsuits (Dunbar et al. 1995). Restrictions on private litigation under PSLRA may reduce such dead-weight losses and benefit shareholders. However, restrictions that make it harder for litigants to file meritorious lawsuits also reduce the incentives of corporate officers and external professional advisors (e.g., auditors and investment bankers) to disclose information truthfully (Seligman 1994a), and the resulting information uncertainty may harm shareholders (Lev 1995). Additional PSLRA restrictions may also make it harder for shareholders to recover future investment losses from external professional advisors, and from directors' and officers' liability-insurance providers.

This study examines stock price reactions to events that changed the likelihood of the passage of PSLRA and other related events, to assess whether shareholders considered PSLRA beneficial or harmful. Our analysis focuses on four industries that are most susceptible to securities lawsuits and that consequently are most likely to be affected by the passage of PSLRA. These high-litigation-risk industries are computers, electronics, pharmaceuticals/biotechnology, and retailing (Francis et al. 1994a).

Spiess and Tkac (1997) (hereafter ST) and Johnson et al. (2000) (hereafter JKN) also examine stock price reaction to PSLRA for firms in these industries. They focus on the price reaction to the presidential veto late on the night of 12/19/95 and the congressional votes to override the veto on 12/20/95 (House) and 12/22/95 (Senate). They observe significantly negative abnormal returns for firms in high-litigation-risk industries on 12/18/95, which they attribute to veto rumors, and significantly positive abnormal returns on 12/20/95, which they attribute to the House vote to override the veto. As a result, they conclude that shareholders considered PSLRA beneficial.

We present evidence that is inconsistent with their conclusion. First, we show that conventional statistical procedures overstate the significance of the excess returns on 12/18/95. Second, based on a detailed analysis of press coverage, and results of intertemporal correlation analysis following the market price reversal methodology of Smith (1981) and Collins et al. (1982), we argue that the positive abnormal return observed on 12/20/95 is more likely a response to the presidential veto than to the House override. Third, we undertake a more comprehensive examination of the events during the almost year-long debate on PSLRA and find that significant abnormal returns for events other than the veto suggest that shareholders considered PSLRA harmful. Fourth, we observe significantly negative cumulative abnormal returns during the last three weeks, when the final version of the bill received the congressional approval, presidential veto, and congressional override of the veto, taking the probability of the bill's passage from less than one to one. Fifth, we find a significantly negative stock price reaction to the defeat of an initiative, Proposition 211, intended to reverse many of PSLRA's key provisions (Bencivenga 1996); these abnormal returns are positively correlated with abnormal returns related to the passage of PSLRA. Finally, we find significantly negative abnormal returns to the last securities-litigation-related major judicial or legislative event before the enactment of PSLRA in

1995—the 1994 Supreme Court decision that made it harder for shareholders to sue professional advisors, such as auditors and investment bankers, suspected of willfully aiding and abetting securities violations (Seligman 1994b). Although the timing of the presidential veto and the House vote to override the veto make it difficult to unambiguously interpret the returns on 12/20/95, the entire body of our results is more consistent with the inference that, on average, shareholders in the high-litigation-risk industries considered PSLRA harmful.

There is little evidence in the literature on the effect on shareholders of changes in the U.S. securities-litigation environment. Our study (along with ST and JKN) addresses this void by documenting stock price consequences of PSLRA and other related events. Other studies have attempted to document the effect of PSLRA on shareholders by comparing patterns of securities litigation before and after the Act (e.g., Grundfest and Perino 1997; SEC 1997a). However, such an approach requires several years' data on actual cases and their outcomes under PSLRA to draw reliable conclusions about the Act's effect. Moreover, such an approach requires a subjective determination of the merit of lawsuits because PSLRA benefits shareholders if nonmeritorious lawsuits are curbed to a greater extent than are meritorious ones. The event-study methodology we use avoids these difficulties by providing a more timely and objective assessment of the Act's effect on shareholders.

We have organized the rest of the paper as follows. Section II describes the PSLRA, its effect on shareholders, and events leading to the passage of the Act. Section III explains our research design, and Section IV discusses the results. Section V discusses other events related to PSLRA—Proposition 211 (which sought to overturn key provisions of PSLRA) and Supreme Court decisions related to securities litigation. Section VI shows that our results are robust to using a much broader sample with a continuous litigation-risk measure based on the Jones and Weingram (1996) model. Section VII provides concluding remarks.

II. PRIVATE SECURITIES LITIGATION REFORM ACT

After paying big settlements in cases related to the savings and loan crisis, accountants lobbied for tort reform, and by the early 1990s there was a widespread belief that the litigation explosion had gotten out of hand. Bills introduced in Congress led to the Private Securities Litigation Reform Act of 1995, which contains several provisions (Hamilton 1996). First, the Act makes it more difficult to file frivolous lawsuits by requiring plaintiffs to plead specific facts alleging that management intentionally committed fraud, and it imposes penalties (normally the payment of the other parties' attorneys' fees) on plaintiffs if the lawsuit is found to be frivolous. Second, the Act provides companies a safe harbor for forward-looking disclosures that are accompanied by "meaningful cautionary statements." Third, it moves toward proportional liability instead of joint and several liability for defendants not found to have knowingly violated securities laws (King and Schwartz 1997). This limits the liability of deep-pocket defendants, such as auditors and underwriters, who previously had been liable for damages up to the full amount of the settlement or judgment even if they were responsible for only a small portion of the blame. Finally, the Act allows large shareholders to petition the court to be appointed as lead plaintiff, which is intended to reduce plaintiff lawyers' ability to file frivolous lawsuits for their own gain.

Effect of the Act on Shareholders

Most large securities-fraud cases are class actions filed by lawyers on behalf of investors who bought shares from other investors when the price was, allegedly, artificially high due to misstatements by the company. The sellers reap an undeserved windfall, but cannot be sued because they are innocent. So the injured investors sue the company and its officers and professional advisors who are responsible for the misleading statements. There is a considerable debate whether PSLRA, which restricts private litigants' ability to sue for securities fraud, benefits or hurts shareholders (Avery 1996).

Dead-Weight Losses of Litigation

Proponents of the Act argue that much of the pre-PSLRA litigation was frivolous. According to this view, legal defense costs were high, and it was in the defendants' interest to settle rather than litigate. Therefore, plaintiff lawyers filed nuisance lawsuits simply to receive a settlement, regardless of the actual merits of the case. The company's shareholders bore the cost of securing insurance coverage against losses from such lawsuits, and the costs of defense and settlement not reimbursed by insurers. Shareholders also suffered other adverse effects of the litigation, as managers diverted attention from other issues, companies were less willing to enter industries where the potential for lawsuits was high, and directors, auditors, and professional advisors were hesitant to associate themselves with such industries. The threat of such litigation may have also hurt shareholders by making managers reluctant to disclose forward-looking information (Avery 1996). The dead-weight losses of frivolous litigation suggest that high-litigation-risk firms' shareholders would react *positively* to PSLRA.

Deterrence Effect of Litigation

Opponents of PSLRA argued that the courts could dismiss and sanction nonmeritorious securities litigation even under the old law (Seligman 1994a). PSLRA's new restrictions may discourage plaintiffs from filing even meritorious suits. President Clinton also expressed a similar concern. His veto message stated that the bill would "have the effect of closing the courthouse doors on investors who have legitimate claims" (Hamilton 1996, 701).[1] Opponents of PSLRA are concerned that the law may provide too much immunity to corporate officers and professional advisors, reducing their incentives to disclose timely and accurate information to shareholders.[2] The resulting information uncertainty would harm shareholders (Lev 1995). This view suggests that high-litigation-risk firms' shareholders would react *negatively* to PSLRA.

Unexpected Loss of Insurance Value

PSLRA would have reduced shareholders' expected recovery of *post-PSLRA* investment losses from outside professionals, and from directors' and officers' liability-insurance providers. Firms may have renegotiated fees/premiums paid to these parties to reflect the lower likelihood of lawsuits due to PSLRA. However, to the extent that these fees/premiums are not completely adjusted, these parties would benefit at the expense of the shareholders, suggesting that high-litigation-risk firms' shareholders would react *negatively* to PSLRA.[3]

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[1] Grundfest and Perino (1997) show that after PSLRA plaintiffs must, on average, demonstrate more dramatic wrongdoing to satisfy the new federal pleading standard. Specifically, they report that the average daily-stock-price decline on the disclosure of wrongdoing increased from 19 percent to 31 percent after the enactment of PSLRA.

[2] Seligman (1994a) points out that the SEC has limited resources for enforcing the mandatory disclosure system. Usually, the SEC obtains only consent decrees, whereby corporate wrongdoers agree not to do certain things in the future. Thus, the most effective deterrent preventing corporate officers and others from defrauding shareholders is private litigation.

Events Leading to the Reform Act

The main difficulty with measuring the effects of legislative events on security prices is identifying when the market first anticipates the effects of the events (Binder 1985). The road to the Private Securities Litigation Reform Act of 1995 was "long and winding" (Avery 1996). The capital market, during the almost year-long legislative debate, would have anticipated the effects of most of the legislative events before they occurred. To mitigate this measurement problem, we follow the Schipper and Thompson (1983) strategy of considering legislative events that are likely to have significantly altered expectations about the bill's passage. For PSLRA, these events include the three sets of full House and Senate votes, and the presidential veto. Table 1 lists these events, and indicates whether they increased or decreased the likelihood of the passage of PSLRA. Unfortunately, news of the veto and the House override of the veto reached the market on the same day (12/20/95), and the two events have opposite effects on the likelihood of PSLRA's passage. We interpret subsequent analysis of the financial press reports as indicating that the veto was more surprising than was the House vote. Thus, for ease of exposition, the prediction listed in Table 1 is based on the assumption that the market reaction on 12/20/95 is due primarily to the news of the veto, although in light of the ambiguity induced by the confounding events, we place a question mark next to it.

To provide a more comprehensive analysis, we also examine abnormal returns for several other events during the legislative debate on PSLRA: the introduction of the bills in the Congress; congressional committee and subcommittee votes; conference committee deliberations; the Securities and Exchange Commission's (SEC's) position on the legislation; President Clinton's indicated willingness to sign the bill; and the rumor of presidential veto. Since for many of these events it is difficult to assess whether the event increased or decreased the likelihood of the passage of PSLRA, we do not make predictions for this set of events. Table 1 also lists these events.[4]

Republicans won control of Congress in 1994, and soon thereafter introduced the Common Sense Legal Reform Act in the House, and the Private Securities Litigation Reform Act of 1995 in the Senate. SEC Chairman Arthur Levitt warned that changes envisaged by

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[3] If shareholders fail to file a lawsuit for pre-PSLRA investment losses during the less restrictive pre-PSLRA regime, the firm's value may increase from the passage of PSLRA. This argument predicts that stocks of high-litigation-risk firms would react *positively* to PSLRA. However, this prediction is countered by the following arguments. First, the directors' and officers' liability-insurance providers generally cover the potential damage claims against firms. Hence, it is the insurance provider, rather than the firm, who may benefit from the passage of PSLRA. Second, shareholders with pre-PSLRA investment losses had the incentive to sue the firms before the enactment of PSLRA, and in fact, lawsuits surged during the weeks before the passage of PSLRA (Schmitt 1996). To the extent that shareholders filed a lawsuit for pre-PSLRA investment losses before PSLRA's effective date, its passage may not have affected firm value.

[4] We do not examine stock price reaction to congressional hearings and floor debates on PSLRA. We think that the market largely anticipates the substance of these events. For example, on 1/19/95, the House subcommittee on Telecommunication and Finance held the first hearing on the Securities Litigation Reform Act. Avery (1996) reports that three witnesses testified in favor of the bill: a representative of the American Business Conference, the chairman of a high-technology company, and Professor R. Fischel of the University of Chicago Law School; and that William S. Lerach, on behalf of the National Associations of Securities and Commercial Law Attorneys, testified against. The market is unlikely to have been surprised by their testimonies, given the position of the organizations that the witnesses represented, and in the case of Professor Fischel, given his career as a highly paid consultant to corporate defendants in securities-fraud cases (Henriques 1995a). Similar concerns apply to other congressional hearings and floor debates.

Republicans would impair the nation's securities regulation system by undercutting investors' right to sue companies that inflate the value of their stock (Avery 1996). Both the House and the Senate amended the bills to address some of Levitt's concerns. On 3/8/95 the House approved its bill with a vote of 325–99, and on 6/28/95 the Senate approved its bill with a vote of 70–29. The House and Senate subsequently reconciled the differences in the two bills adopted by their respective chambers, mostly to conform to the more moderate Senate version. On 12/5/95, the Senate approved the bill, 65-30, and the House approved the bill the next day with a vote of 320–102.

The President had until December 19 to sign or veto the bill. Although Congress did not completely address the administration's objections in the bill, it passed; a few days after the House and Senate votes, the president "privately" conveyed his intention to sign the bill (Frisby and Taylor 1995a). However, on the night of December 19, President Clinton vetoed the bill, indicating that he disapproved of certain provisions. The stock market reacted to the veto on December 20. On the same day—December 20—as could be expected given the earlier lopsided vote on the bill, the House voted 319–100 to override the veto. The Appendix provides quotes from several newspaper articles suggesting that President Clinton's veto on the night of December 19 was more surprising than was the House vote to override the veto on December 20. Thus, in our opinion, the stock price reaction of high-litigation-risk firms on December 20 is more likely attributable to the veto than to the House vote.[5] It was not certain, however, that the Senate would be able to override the veto (Frisby and Taylor 1995b). The following day, President Clinton lobbied intensely to defeat the bill, but with little success (Frisby and Taylor 1995c). On December 22, the Senate overrode the veto 68–30, a margin of only two votes.

III. RESEARCH DESIGN

To examine shareholders' response to PSLRA, we estimate, for PSLRA event dates, abnormal returns for a portfolio of firms in four industries that Francis et al. (1994a) assert experienced relatively high incidence of litigation during 1988–1992: computers (SIC codes 3570–3577 and 7370–7374), electronics (SIC codes 3600–3674), pharmaceuticals/biotechnology (SIC codes 2833–2836 and 8731–8734), and retailing (SIC codes 5200–5961).[6] Following Ryngaert and Netter (1988) and Karpoff and Malatesta (1989), we employ the following model to compute abnormal returns:

$$R_t = a_0 + a_1 R_{mt} + \Sigma a_{2i} D_i + u_t, \tag{1}$$

where R_t is the daily return of a portfolio of high-litigation-risk firms, R_{mt} is the value-weighted market index, and D_i is a dummy variable that takes a value of 1 when the day corresponds to event date i, and 0 otherwise. The coefficient a_{2i} represents the average abnormal return of high-litigation-risk firms on event date i. Since all our PSLRA events occur in 1995, we estimate equation (1) using daily-return data for 1995. The portfolio of high-litigation-risk firms on a given day consists of all firms in the four industries for which daily returns data are available on the CRSP database. Because of missing returns data, the number of firms in the daily portfolios is not the same; the minimum (maximum) number of firms is 1,443 (1,589). We also estimate equation (1) separately for each of the four industries. The minimum (maximum) numbers of firms in the computers, electronics, pharmaceuticals/biotechnology, and retailing portfolios are 492 (579), 430 (484), 74 (79), and 441 (450), respectively.

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[5] ST and JKN attribute the December 20 stock price reaction to news of the House vote. Sections IV and V of our paper provide additional evidence to distinguish between the two interpretations.

[6] O'Brien and Hodges (1993) and Jones and Weingram (1996) also report that high-tech companies were involved in a disproportionately large number of securities-fraud class-action cases. Grundfest and Perino (1997) report that this continues to be the case after PSLRA. We repeat our analysis in a broader sample of firms, using an alternative measure of litigation risk. Results reported in Section VI are consistent with inferences from our primary analysis.

Jain (1986) shows that event studies in which firms have the same event date are susceptible to overstated (understated) significance levels when the absolute value of market returns on the event date is high (low). To control for this bias, we conduct a randomization test (Noreen 1989). For each of the event dates, we randomly select 100 dates during the 1976–1995 period with market returns within 0.05 percent of the returns on the test dates. For each of the random dates, we obtain abnormal returns by estimating equation (1). We use daily returns for 250 days preceding the selected random date for estimation purposes. We estimate the significance level of the abnormal return on the test date based on the distribution of the estimated abnormal returns for the 100 random days. Specifically, the two-tailed p-value is the proportion of random days on which the absolute value of the abnormal return exceeds the abnormal return we observe on the test date.

Noreen (1989, Appendix 2A) also demonstrates that the randomization test is just as powerful as the conventional parametric test. Consequently, we base our conclusions about the significance of daily abnormal returns on the randomization p-values, although we also report significance levels from conventional t-tests for comparability with other studies.[7]

IV. RESULTS

Regression Results

Table 1 reports OLS estimates of equation (1) for the portfolio of all high-litigation-risk firms. Of the six dates for which we predict whether the event will increase or decrease the likelihood of passage of PSLRA, abnormal returns are significantly different from zero at better than the 10 percent level (two-tailed test based on the randomization p-values) on four dates. Abnormal return on 6/28/95 (Senate passes its bill) is −0.58 percent ($p = 0.10$); on 12/5/95 (Senate approves the conference committee bill) is −0.71 percent ($p = 0.08$); on 12/6/95 (House approves the conference committee bill) is −0.61 percent ($p = 0.09$); and on 12/20/95 (President vetoes bill and House overrides veto) is 1.12 percent ($p = 0.02$).[8] The first three dates unambiguously increase probability of passage, and market reacted negatively, inconsistent with ST's and JKN's interpretations. Although we regard 12/20/95 as decreasing the probability of passage (for reasons spelled out later), interpretation of the 12/20/95 return itself is inconclusive because of the two

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[7] We do not conduct randomization procedures to assess significance levels for cumulative abnormal returns. It would require the random dates to have consecutive days with market returns similar to the market returns on each of the consecutive test days. It would not be possible to obtain sufficient observations. Moreover, with cumulative returns, the effect of the bias due to an unusually small or large market return on a particular date is likely to be relatively small.

[8] ST's and our sample firms are from the same industries. For the dates that are common between ST (Table 2) and our Table 1, abnormal returns are very similar in magnitude. ST do not report t-statistics or p-values (other than to indicate that they are below the 5 or 10 percent levels), making it difficult to compare significance levels across the two studies. There are two dates in Table 1 where our conventional p-values suggest that the abnormal returns are significant, whereas ST classifies abnormal returns on these two dates as insignificant. On 12/5/95 and 12/11/95, our conventional p-values are 0.10, whereas ST indicates p-values of greater than 10 percent (they do not report the actual values). However, the magnitudes of abnormal returns on these two dates are very similar across the two studies (−0.80 percent and −0.86 percent in ST, and −0.71 percent and −0.71 percent in our study). JKN's sample is different from both ST's and our sample, however, except for one event date, their results (JKN, Table 1) are similar to ST's and ours for events that are common across our studies. We discuss the exception in the next section.

TABLE 1
Results from Ordinary Least Squares Regression of Daily Portfolio Returns for High-Litigation-Risk Firms on Market Index and Dummy Variables for Event Dates Leading to the Enactment of PSLRA
Estimation Period is the 252 Trading Days in the Year 1995[a]

Model: $R_t = a_0 + a_1 R_{mt} + \Sigma a_{2i} D_i + u_t$[b]

Event date, i	Prediction[c]	a_{2i} Coeff.	a_{2i} (t-stat., p-value)	Randomization p-value[d]	Event Description (Source)[e]
Intercept		0.07	(2.43, 0.01)***		
R_{mt}		1.01	(16.69, 0.00)***		
1/4/95		0.02	(0.06, 0.95)	0.74	Common Sense Legal Reform Act introduced in the House. (*CPR*)
1/18/95		0.28	(0.67, 0.50)	0.45	Private Securities Litigation Reform Act of 1995 introduced in the Senate. (*CPR*)
1/25/95		−0.70	(−1.65, 0.10)	0.05**	SEC Chief warns legislators that changes envisaged by Republicans would impair the nation's securities regulation system. (*NYT*)
2/9/95		0.93	(2.17, 0.03)	0.04**	Legislators modify bill to meet some of SEC's objections. (*WP*)
2/14/95		0.07	(0.17, 0.87)	0.83	Finance Subcommittee passes the bill, 16-10. (*CPR*)
2/15/95		0.02	(0.05, 0.96)	0.81	SEC rejects revised bill. (*WP*)
2/16/95		0.09	(0.20, 0.84)	0.94	House Commerce Committee passes the bill, 32-10. (*CPR*)
3/8/95	I	0.00	(0.01, 0.99)	0.99	House passes its bill, 325-99. (*WSJ*)
5/19/95		−0.06	(−0.14, 0.89)	0.79	SEC asks Senator D'Amato, Chairman, Senate Banking Committee, to amend the safe harbor provision in the House bill, which would have given sweeping protection to companies. (*WSJ*)
5/24/95		−0.12	(−0.29, 0.77)	0.69	Senator D'Amato complies. (*WSJ*)
5/25/95		0.14	(0.34, 0.73)	0.76	Senate Banking Committee approves the bill, 11-4. (*WSJ*)
6/28/95	I	−0.58	(−1.37, 0.17)	0.10*	Senate passes its bill, 70-29. (*WSJ*)

(*Continued on next page*)

TABLE 1 (Continued)

Event date, i	*Prediction*[c]	a_{2i} *Coeff.*	a_{2i} *(t-stat., p-value)*	*Randomization p-value*[d]	*Event Description (Source)*[e]
10/24/95		−0.20	(−0.46, 0.65)	0.55	House and Senate staffers hammer out a compromise. (*WSJ*)
11/16/95		−0.23	(−0.54, 0.59)	0.41	SEC approves safe harbor provisions, making a presidential veto less likely. (*WSJ*)
11/28/95		0.06	(0.14, 0.89)	0.94	Conference Committee finalizes the bill.
12/5/95	I	−0.71	(−1.66, 0.10)	0.08*	Senate approves the bill, 65-30. Clinton is undecided. (*WSJ*)
12/6/95	I	−0.61	(−1.44, 0.15)	0.09*	House approves the bill, 320-102. (*WSJ*)
12/11/95		−0.71	(−1.66, 0.10)	0.05**	President indicates willingness to sign the bill. (*WSJ*)
12/18/95		−1.38	(−3.14, 0.00)	0.16	Clinton is officially undecided. Rumors are that he will veto the bill. (*National Journal's Congress Daily*)
12/20/95	D (?)	1.12	(2.62, 0.01)	0.02**	President vetoes bill (on 12/19/95 after market close), and House overrides veto, 319-100. (*WSJ*)
12/21/95		−0.06	(−0.13, 0.90)	0.78	Senate poised to override the veto. (*WSJ*)
12/22/95	I	0.39	(0.91, 0.36)	0.20	Senate overrides veto, 68-30. (*WSJ*)
Adj. R^2			0.57		

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*, **, *** indicate significance at 10, 5, and 1 percent level or better, respectively (two-tailed test).

[a] The portfolio of high-litigation-risk firms on a given day consists of all firms in the following four industries for which daily returns data are available: computers (SIC codes 3570–3577 and 7370–7374), electronics (SIC codes 3600–3674), pharmaceuticals/biotechnology (SIC codes 2833–2836 and 8731–8734), and retailing (SIC codes 5200–5961). The number of firms in the portfolio ranges from 1,443 to 1,589.

[b] Variable definitions:

R_t = high-litigation-risk portfolio daily stock return in percentage;
R_{mt} = CRSP daily value-weighted market return in percentage; and
D_i = 1 if the day corresponds to event date i, and 0 otherwise.

[c] Predictions: Increase (I) or decrease (D) in the likelihood of the passage of PSLRA.

[d] Randomization p-values are based on the distribution of abnormal returns for 100 random days with market returns within plus or minus 0.05 percent of that of the test day; the exception is 12/18/95 (see footnote 10).

[e] Sources: *CPR: Congressional Press Releases; WSJ: Wall Street Journal; NYT: New York Times; WP: Washington Post.*

confounding events on this day. Although we regard 12/20/95 as decreasing the probability of passage (for reasons spelled out later), interpretation of the 12/20/95 return itself is inconclusive because of the two confounding events on this day.

Abnormal returns for the portfolio of high-litigation-risk firms are significant for three other dates. On 1/25/95, the SEC chairman indicated that some of the proposed changes to the law would make it too difficult for private litigants to sue, and would "impair the nation's securities regulation system" (Henriques 1995b). The negative abnormal return, −0.70 percent ($p = 0.05$), suggests that shareholders reacted adversely to the SEC's criticism. The abnormal return on 2/9/95, when the House legislators watered down the bill to meet some of the SEC's objections, is 0.93 percent ($p = 0.04$). This positive market reaction is consistent with shareholders' preference for a bill that would make it less difficult to sue for fraud. The abnormal return on 12/11/95, when President Clinton indicated his willingness to sign the bill, is −0.71 percent ($p = 0.05$), consistent with shareholders' unfavorable view of PSLRA.

For each of the dates for which the full sample of high-litigation-risk firms exhibits a coefficient that is significantly different from zero in Table 1, Table 2 reports abnormal returns for each industry sample separately. Of 28 coefficients (seven event dates times four industry portfolios), signs of 26 coefficients are consistent with the signs of the coefficients observed for the full sample. The consistency across the four high-litigation-risk industries provides confidence that the results stem from the litigation-related events.

Alternative Interpretation of the Results

ST and JKN argue that on December 18 there was a negative stock price reaction due to a rumor that President Clinton would veto the bill. They further argue that the positive stock price reaction on December 20 is due primarily to the House vote to override the veto. In contrast to ST and our study, JKN report a significantly positive abnormal return on December 22, attributing it to the Senate override of the veto. ST and JKN conclude that shareholders of high-litigation-risk firms reacted favorably to the passage of PSLRA. We reexamine their evidence and conclusion.

December 18 Abnormal Returns

ST and JKN conclude that abnormal returns on December 18 are significantly negative based on p-values from a conventional t-test, and our Table 1 also reports that a conventional p-value for December 18 suggests statistical significance. However, Jain (1986) shows that the significance level of market-model-based abnormal returns is likely to be overstated when market return on the event date is large (in absolute value)—and the absolute market return on December 18 is relatively large, at −1.7 percent.[9] Table 1 shows that the randomization p-value for the abnormal return of high-litigation-risk firms is 0.16.[10] This result suggests that we cannot reliably conclude that a PSLRA-related event affected stock prices on 12/18/95, which is inconsistent with ST's and JKN's inference that high-litigation-risk

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[9] The main reasons cited for the large decline in stock prices were growing concerns about the Washington budget imbroglio, which had already partially shut down the government, and a weakening economy with more signs of earnings difficulty (Kansas 1995).

[10] The randomization procedure is similar to the one described in Section III, with the following exception. There were so few dates with such large absolute market returns that we had to use market returns within plus or minus 0.10 percent, instead of 0.05 percent, from the test-date market return. Even so, for the entire 1976–1995 period, we found only 37 days with market returns between −1.8 and −1.6 percent. For sensitivity analysis, we select random days between 1976 and 1995 on which the market return is within plus or minus 0.15 percent (0.20 percent, 0.25 percent) of −1.7 percent, and obtain 54 (71, 96) such days. The randomization p-value turns out to be 0.15 (0.17, 0.14), which is very close to the 0.16 value reported in Table 1.

TABLE 2

For Each Industry in High-Litigation-Risk Category, Results from Ordinary Least Squares Regression of Daily Portfolio Returns on Market Index and Dummy Variables for PSLRA Event Dates with Significant Abnormal Returns in Table 1 Estimation Period is the 252 Trading Days in the Year 1995[a]

Model: $R_t = a_0 + a_1 R_{mt} + \Sigma a_{2t} D_t + u_t$[b]

Event Description	Independent Variable or Event Date	*Computers*		*Electronics*		*Pharmaceuticals/Biotech*		*Retailing*	
		Coefficient (t-stat, p-value)	*Randomization p-value*[c]	*Coefficient (t-stat, p-value)*	*Randomization p-value*[c]	*Coefficient (t-stat, p-value)*	*Randomization p-value*[c]	*Coefficient (t-stat, p-value)*	*Randomization p-value*[c]
	Intercept	0.09 (2.41, 0.02)**		0.08 (2.05, 0.04)**		0.09 (2.33, 0.02)**		0.03 (1.24, 0.21)	
	R_{mt}	1.24 (15.54, 0.00)***		1.21 (15.15, 0.00)***		0.61 (7.21, 0.00)***		0.60 (10.72, 0.00)***	
SEC cautions against Bill	1/25/95	−1.11 (−1.96, 0.05)	0.01***	−0.63 (−1.12, 0.26)	0.14	−0.05 (−0.08, 0.94)	0.85	−0.44 (−1.11, 0.27)	0.14
Bill revised to suit SEC	2/9/95	0.95 (1.69, 0.09)	0.10*	1.23 (2.19, 0.03)	0.03**	−0.01 (−0.01, 0.99)	0.99	0.77 (1.96, 0.05)	0.07*
Senate passes its Bill	6/28/95	−0.66 (−1.17, 0.24)	0.18	−1.11 (−1.99, 0.05)	0.02**	−0.26 (−0.44, 0.66)	0.59	−0.01 (−0.03, 0.98)	0.98
Senate passes revised Bill	12/5/95	−0.80 (−1.43, 0.15)	0.12	−1.13 (−2.01, 0.04)	0.03**	0.27 (0.46, 0.65)	0.61	−0.31 (−0.80, 0.42)	0.30
House passes revised Bill	12/6/95	−0.69 (−1.22, 0.22)	0.12	−0.70 (−1.24, 0.21)	0.15	−0.40 (−0.68, 0.50)	0.34	−0.47 (−1.20, 0.23)	0.17
President willing to sign	12/11/95	−0.67 (−1.19, 0.23)	0.17	−0.54 (−0.96, 0.34)	0.22	−0.26 (−0.43, 0.67)	0.65	−1.02 (−2.60, 0.01)	0.01***
Veto/House Override	12/20/95	1.41 (2.50, 0.01)	0.01***	0.99 (1.76, 0.08)	0.05**	1.25 (2.10, 0.04)	0.02**	0.87 (2.20, 0.03)	0.06*
	Adj. R^2	0.54		0.50		0.24		0.35	

(*Continued on next page*)

TABLE 2 (Continued)

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*, **, *** indicate significance at 10, 5, and 1 percent level or better, respectively (two-tailed test).

[a] An industry portfolio on a given day consists of all firms for which daily returns data are available. The industry composition is as follows: computers (SIC codes 3570–3577 and 7370–7374), electronics (SIC codes 3600–3674), pharmaceuticals/biotechnology (SIC codes 2833–2836 and 8731–8734), and retailing (SIC codes 5200–5961). The number of firms in the computer, electronics, pharmaceutical/biotechnology, and retailing industry portfolios ranges from 492 to 579, 430 to 484, 74 to79, and 441 to 450, respectively.

[b] Variable definitions:

R_t = industry portfolio daily stock return in percentage;

R_{mt} = CRSP daily value-weighted market return in percentage; and

D_i = 1 if the day corresponds to event date i, and 0 otherwise.

[c] Randomization p-values are based on the distribution of abnormal returns for 100 random days with market returns within plus or minus 0.05 percent of that of the test day.

TABLE 3

Spearman Correlations of Abnormal Returns of Firms in High-Litigation-Risk Industries[a] on the Possible Veto News Dates (12/18/95 and 12/20/95) with Abnormal Returns on Other Veto-Related Event Dates

		Veto Rumor 12/18/95			*Veto/House Override 12/20/95*		
		Predicted Sign			*Predicted Sign*		
Event Date	*Description*	*ST's and JKN's Interpretation*	*This Study's Interpretation*	*Correlation*[b] *(p-value)*	*ST's and JKN's Interpretation*	*This Study's Interpretation*	*Correlation (p-value)*
12/11/95	President willing to sign Bill	–	no prediction	+0.03 (0.18)	+	–	−0.08 (0.00)***
12/18/95	Veto rumor				–	no prediction	+0.00 (0.94)
12/20/95	Veto/House override	–	no prediction	+0.00 (0.94)			
12/22/95	Senate override	–	no prediction	−0.01 (0.59)	+	–	−0.04 (0.08)*

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*, **, *** indicate significance at 10, 5, and 1 percent level or better, respectively (two-tailed test).

[a] The number of sample firms on a given day include all firms in the following four industries for which daily returns data are available: computers (SIC codes 3570–3577 and 7370–7374), electronics (SIC codes 3600-3674), pharmaceuticals/biotechnology (SIC codes 2833–2836 and 8731–8734), and retailing (SIC codes 5200–5961). The sample size used for computing the correlations ranges from 1,587 to 1,589.

[b] Two-tailed p-values in parentheses.

reacted positively to the Senate vote to override the veto. ST (Table 2) do not obtain a significant result for this date, 0.48 percent (p-value not reported, but insignificant at the 10 percent level), nor do we, 0.39 percent (p = 0.20). JKN's result is driven by an abnormal return for their pharmaceutical/biotechnology industry sample (2.82 percent, p = 0.00). For this industry, however, ST (Table 2) report insignificant abnormal returns, 0.23 percent (p-value not reported), as do we, 0.68 percent (p = 0.29).

JKN's pharmaceuticals/biotechnology sample has 191 firms, ST's has 65 firms, and ours has 74 to 79 firms. The main reason for the difference in the sample sizes is that unlike ST's and our samples, JKN's sample includes firms on CRSP with SIC code 2830.[14] CRSP has not assigned these firms to one of the specific four-digit SIC codes starting with 283: 2833, 2834, 2835, and 2836 (1987 SIC manual). Accordingly, the firms in JKN's pharmaceuticals/biotechnology sample should have litigation-risk characteristics similar to those with SIC codes 2833–2836 in ST's and our pharmaceuticals/biotechnology samples.

However, JKN observe a significant abnormal return on 12/22/95 only in their pharmaceuticals/biotechnology sample (and not across all of their high-litigation-risk industries), and even this appears to be attributable to a subset of the pharmaceuticals/biotechnology sample (because neither ST nor we observe a significant abnormal return for our pharmaceuticals/biotechnology samples on 12/22/95). Thus, evidence that the market reacted positively to the Senate vote is mixed at best.[15]

Even though the financial press expected that the House would easily override the veto, it suggested that there was uncertainty about the Senate override (see the Appendix). If the positive abnormal return on 12/20/95 is due to the veto, then one might question why the abnormal return on 12/22/95, when the Senate voted to override the veto, is not significantly negative.[16] One explanation is that the Senate's veto-override process watered down a key provision. In his veto message, President Clinton indicated that the bill sent to him submitted plaintiffs to a pleading requirement that was more severe than the Second Circuit standard for pleading. The Second Circuit standard considers reckless conduct sufficient for pleading, and the plaintiff is not required to show that the defendant acted willfully. While urging an override of the President's veto, Senator Dodd (one of the bill's sponsors) said on the floor of the Senate that, contrary to the President's interpretation, the bill does not depart from the Second Circuit standard for pleading (141 *Congressional Record* S19, 068 [1995]). Senator Domenici reiterated this point (141 *Congressional Record* S19, 150

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[14] We thank JKN for providing their sample.

[15] Except for 12/22/95, none of our results are sensitive to whether we exclude firms with SIC code 2830 in the pharmaceuticals/biotechnology sample. However, on 12/22/95 the pharmaceuticals/biotechnology sample that includes firms with SIC code 2830 yields an abnormal return of 2.86 percent. This is about six times the next-highest abnormal return of 0.50 percent for the computer firms (JKN, Table 1, also report a similar result). For other event dates with significant abnormal returns for the full sample, the highest industry abnormal return is never more than twice the next-highest industry abnormal return. Thus, except for 12/22/95, the magnitude of abnormal returns for the full sample of high-litigation-risk firms is not dominated by one industry.

We decided to report in this paper the results for the sample excluding firms with SIC code 2830 for the following reason. Our sample, like ST's, includes firms on CRSP in industries with four-digit SIC codes that Francis et al. (1994a) suggest experience high litigation rates. Thus, our sample is directly comparable to ST's. We elected to use a sample comparable to ST's rather than sample comparable to JKN's because JKN's sample excludes two industries that Francis et al. (1994a) consider high litigation risk—the electronics and retailing industries. Thus, JKN's full sample contains only 489 firms compared to 1,485 firms in ST and 1,443 to 1,589 firms in our study.

[16] Since 12/22/95 was the Friday before Christmas, trading was lighter than usual—volume on New York Stock Exchange was 289.6 million shares, compared with 415.8 million shares the day before. Thus, news of the Senate vote may not have been completely impounded in stock prices on 12/22/95. However, the abnormal returns on 12/22/95 and when the market reopened on 12/26/95 together are also insignificant, −0.17 percent (p = 0.66).

[1995]). Thus, when the Senate overrode the veto, the bill's legislative history supported a more lenient standard for pleading. An SEC brief, two court rulings, and a statement by President Clinton are consistent with this interpretation. Specifically, the SEC filed a brief in a recent lawsuit to indicate that the legislative history weakening the bill during the veto override should be considered in the judicial decision (SEC 1997b). Also, at least two court rulings have relied on Senator Dodd's statement to interpret the pleading standard in PSLRA (*Zeid* v. *Kimberly* and *Epstein* v. *Itron*). Finally, President Clinton also says: "(A)fter my veto, the bill's supporters made it clear that they did in fact intend to codify the Second Circuit standard. After this important assurance, the bill passed over my veto" (*U.S. Newswire* 1998).

Cumulative Returns during December 1995

We conduct an alternative analysis that does not require (a necessarily subjective) identification of specific days on which value-relevant information regarding the increase or decrease in the probability of the passage of PSLRA is disclosed. Specifically, we examine the high-litigation-risk firms' cumulative abnormal returns (CAR) from the day before the congressional vote on the conference committee bill (12/4/95) to the next trading day after its ultimate passage (12/26/95). We include 12/26/95 in this analysis because due to light trading on 12/22/95, stock prices may not have fully impounded the news of the Senate vote until 12/26/95 (see footnote 16). During the 12/4/95 to 12/26/95 period, the probability that PSLRA would pass increased from less than 1.0 to precisely 1.0. Positive (negative) CAR would indicate that shareholders viewed PSLRA as beneficial (not beneficial). Table 4 reports CAR for the full sample of high-litigation-risk firms as well as separately for each of the high-litigation-risk industry portfolios. For the full sample, CAR is negative and significant, −3.48 percent (t = −2.00). CAR is also negative for three of the industry portfolios, although it is significant only for electronics, −4.02 percent (t = −1.80) and retailing, −4.04 percent (t = −2.54). Even if we exclude 12/26/95, CAR for the full sample remains significantly negative, −2.93 percent (t = −1.74). CAR also remains significantly negative for electronics and retailing firms. These results suggest that shareholders viewed PSLRA as not beneficial.[17]

To summarize, we interpret our evidence of significant abnormal returns for certain events leading to the passage of PSLRA, our analysis of intertemporal correlations of abnormal returns between days with expected reversal of the probability of the passage of PSLRA, and the cumulative abnormal returns from the first congressional vote on the conference committee bill to the final Senate override, as more consistent with the conclusion that shareholders viewed PSLRA as harmful. Nonetheless, the confounding veto and House override on December 20 preclude conclusive evidence. We therefore analyze other events related to the securities litigation environment and PSLRA to shed more light on whether shareholders considered restrictions on their ability to bring class-action lawsuits for securities fraud harmful.

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[17] It is unlikely that dilution of the bill is driving the CAR to be negative over the 12/4/95 to 12/26/95 period. The conference committee unveiled on 11/28/95 the version of the bill that was ultimately enacted. Dilution of the bill seems to have occurred only during the Senate's veto-override process, which would be reflected in abnormal returns from 12/21/95 to 12/26/95. Table 4 shows no evidence of a significant change in CAR during that period.

TABLE 4
Cumulative Abnormal Returns for Portfolios of High-Litigation-Risk Firms from the Trading Day before the Congressional Vote on the Conference Committee Bill (12/4/95) to the Trading Day after the Veto Override (12/26/95)[a]

	All High-Risk Firms		*Computers*		*Electronics*		*Pharmaceuticals/ Biotech*		*Retailing*	
Date	*CAR*	*t-stat*	*CAR*	*t-stat*	*CAR*	*t-stat*	*CAR*	*t-stat*	*CAR*	*t-stat*
12/4/95	−0.43	−1.00	−0.22	−0.39	−0.55	−0.98	0.04	0.06	−0.68	−1.71
12/5/95	−1.14	−1.85	−1.03	−1.27	−1.67	−2.12	0.28	0.33	−0.99	−1.75
12/6/95	−1.75	−2.32	−1.71	−1.72	−2.36	−2.44	−0.14	−0.14	−1.45	−2.11
12/7/95	−1.59	−1.82	−1.52	−1.32	−2.06	−1.85	0.15	0.13	−1.48	−1.86
12/8/95	−1.44	−1.48	−1.39	−1.08	−1.74	−1.39	0.17	0.13	−1.49	−1.67
12/11/95	−2.14	−2.01	−2.05	−1.46	−2.27	−1.66	−0.11	−0.07	−2.51	−2.57
12/12/95	−2.54	−2.20	−2.32	−1.52	−2.96	−2.01	0.37	0.23	−2.88	−2.73
12/13/95	−2.65	−2.15	−2.52	−1.55	−3.05	−1.93	0.06	0.04	−2.88	−2.55
12/14/95	−2.50	−1.91	−2.24	−1.29	−3.03	−1.81	−0.45	−0.25	−2.67	−2.23
12/15/95	−3.07	−2.23	−2.99	−1.64	−4.05	−2.30	−0.19	−0.10	−2.65	−2.10
12/18/95	−4.41	−3.06	−4.96	−2.60	−5.00	−2.71	−1.72	−0.87	−3.53	−2.68
12/19/95	−4.40	−2.92	−4.65	−2.34	−4.92	−2.56	−1.47	−0.71	−4.04	−2.93
12/20/95	−3.26	−2.08	−3.20	−1.55	−3.92	−1.96	−0.20	−0.09	−3.16	−2.20
12/21/95	−3.32	−2.04	−3.27	−1.52	−3.97	−1.91	0.86	0.38	−3.45	−2.32
12/22/95	−2.93	−1.74	−2.76	−1.24	−3.77	−1.75	1.51	0.65	−3.06	−1.98
12/26/95	−3.48	−2.00**	−3.44	−1.50	−4.02	−1.80*	2.51	1.05	−4.04	−2.54**

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*, ** indicate significance at 10 and 5 percent level or better (two-tailed test).

[a] The portfolio of high-litigation-risk firms on a given day consists of all firms in the following four industries for which daily returns data are available: computers (SIC codes 3570–3577 and 7370–7374), electronics (SIC codes 3600–3674), pharmaceuticals/biotechnology (SIC codes 2833–2836 and 8731–8734), and retailing (SIC codes 5200–5961). The number of firms in the portfolio ranges from 1,443 to 1,589. The number of firms in the computer, electronics, pharmaceutical/biotechnology, and retailing industry portfolios ranges from 492 to 579, 430 to 484, 74 to79, and 441 to 450, respectively.

[b] CAR = cumulative abnormal returns starting from 12/4/95. Abnormal returns are obtained from OLS regression of daily portfolio returns on market index and dummy variables for dates from 12/4/95 to 12/26/95. Estimation period is the 252 trading days in the year 1995. Model: $R_t = a_0 + a_1 R_{mt} + \Sigma a_{2i} D_i + u_t$, where R_t = Portfolio daily stock return in percentage; R_{mt} = CRSP daily value-weighted market return in percentage; and $D_i = 1$ if the day corresponds to event date i, and 0 otherwise.

V. OTHER EVENTS RELATED TO PSLRA

Proposition 211

California's Proposition 211 proposed to reverse many of PSLRA's key provisions, less than a year after PSLRA was enacted. A negative stock price reaction by high-litigation-risk firms to the defeat of Proposition 211 would suggest that shareholders considered

PSLRA harmful. Also, firms whose stock prices react most negatively to the passage of PSLRA should also have the most negative stock price reaction to the defeat of the proposition. Thus, a positive correlation between abnormal returns associated with the passage of PSLRA and the defeat of Proposition 211 would provide additional evidence that the abnormal returns are due to these litigation-related events, and that the shareholders considered PSLRA harmful. Finally, the direction of the correlation between the abnormal returns on 12/20/95 and the defeat of Proposition 211 can help discern whether the abnormal return on 12/20/95 is more likely due to the veto or to the House vote.

Proposition 211 adopted a "fraud-on-the-market" theory, provided no safe harbor for forward-looking statements, restored joint and several liability for each defendant regardless of the proportionate involvement, and authorized the aiding and abetting liability that the Supreme Court eliminated in 1994. It also allowed punitive damages, prohibited regulation of attorneys' fees, and barred a company from paying the costs of defending its officers who "knowingly or recklessly" engage in deceptive conduct. Though a state ballot measure, it was significant nationally because it permitted lawsuits against any company with shareholders in California (Corcoran 1996; Bencivenga 1996). SEC Chairman Levitt indicated Proposition 211 would have a "nationwide impact" (Donovan 1996), and it was the costliest campaign ever over a California ballot measure (Meyerson 1996).

We examine the stock returns on the day of the Proposition 211 vote (November 5, 1996) and on the day after. On November 5, *State News Brief* (3:05 PM, Eastern Standard Time) predicted that Proposition 211 would fail. Proposition 211 was eventually defeated by a 3 to 1 margin (Abate 1996). The financial press indicated that the greatest uncertainty for the stock market about the election seemed to be the outcome of Proposition 211. The *Associated Press* 11/3/96 (Meyerson 1996) reported that "with most market players betting the elections will produce few changes in Washington, a prickly battle over a California ballot proposal looms as the biggest wild card for Wall Street on Tuesday." The *Washington Post* (Glassman 1996) also made similar observations.

We also consider two other events within a week of the vote. On October 30, 1996, a Field poll indicated that 54 percent of the voters opposed Proposition 211, and that 28 percent were in favor. This was a significant shift since the Field poll in early October, which showed only 31 percent of the voters opposing and 39 percent favoring the proposition (Gunnison 1996). On November 4, the day before the vote, another Field poll indicated that 56 percent of the voters opposed and 25 percent favored the proposition (Yoachum 1996). This news is likely to be informative beyond the October 30 Field poll result, because it indicates that the opposition to Proposition 211 is stable, and the poll result one day before the actual vote is likely to be a more reliable measure of the actual outcome. The events on each of these days indicate a decrease in the likelihood of the passage of Proposition 211, so we classify these events as not being plaintiff-friendly (see panel A of Table 5).[18]

Panel B of Table 5 reports that abnormal returns are negative and significant on November 4, 1996, the day the Field poll results were released, and on November 5, 1996,

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[18] Another recent securities litigation-related legislative event is the passage of Securities Litigation Uniform Standard Act. The law intends to prevent plaintiffs from using state courts to circumvent parts of PSLRA. This law is not expected to have a significant impact on the litigation environment, because the surge in lawsuits at the state level shortly after the passage of PSLRA, which lawmakers used to justify this legislation, was short-lived. The Act thus responds to a problem that does not exist (Painter 1998). We find insignificant abnormal returns for high-litigation-risk firms on the days when news reached the market of the two sets of full House and Senate votes (*Dow Jones News Service* 1998a, 1998b, 1998c, 1998d) and of the President's signing the bill (*U.S. Newswire* 1998); the cumulative abnormal return for these event dates is 0.66 percent, t = 0.65.

Central Bank of Denver, N.A. v. First Interstate Bank of Denver (114 S. Ct. 1439, 1994), the Court set aside 25 years of litigation practice by ruling that rule 10(b)-5 did not prohibit aiding and abetting securities fraud because it contains no language to that effect. This ruling was a response to perceived securities litigation abuses (Avery 1996)—the same concern that prompted Congress to enact PSLRA (Hamilton 1996).

We find a significant stock price reaction only for the narrowly decided (5–4) Supreme Court decision on *Central Bank of Denver*. The lack of significant reaction to the other three Supreme Court decisions is not particularly surprising. For example, Weiss and White (1987) examine seven significant corporate-law-related Delaware court decisions, and find insignificant market reactions to each. The significant stock price reaction to the *Central Bank of Denver* decision underscores both the importance and the surprise element of this decision. Seligman (1994b, 1430) notes it "is the most important federal securities law decision in several years," and that the decision "was an unexpected result." Our analysis, however, cannot rule out the possibility that the other three Supreme Court decisions affected shareholders' wealth, but the market had already anticipated the decisions.

For brevity, Panel B of Table 6 reports the regression results of only the Supreme Court decision on *Central Bank of Denver*. Abnormal return for the portfolio of high-litigation-risk firms is significantly negative, −1.16 percent ($p = 0.01$). Abnormal return is also negative and significant for three high-litigation-risk industries: −1.47 percent ($p = 0.02$) for computers, −1.32 percent ($p = 0.03$) for electronics, and −0.84 percent ($p = 0.04$) for retailing. These results suggest that shareholders reacted negatively to another change in litigation regime that made it harder for shareholders to sue deep-pocket advisors—namely, the Supreme Court decision that 10(b)-5 did not prohibit aiding and abetting securities fraud. This was the last major securities-related judicial or legislative event before the enactment of PSLRA, so the Central Bank of Denver decision results further support the conclusion that shareholders considered restrictions on their rights to sue, such as in PSLRA, to be harmful.

The abnormal return on the day of the *Central Bank* decision is not significantly correlated with the PSLRA-related cumulative abnormal return (PSLRAXR) or with the abnormal return on 12/20/95. A plausible explanation is that PSLRA does not consider the issue of private securities litigation for aiding and abetting, and that firms that were most sensitive to the aiding and abetting issue were not the ones most sensitive to the provisions considered under PSLRA. We also do not observe a significant correlation between abnormal returns for the *Central Bank* decision and the defeat of Proposition 211. Even though Proposition 211 proposed to reverse the effect of the *Central Bank* decision by authorizing the aiding and abetting liability, the proposition had many other provisions that were unrelated to the *Central Bank* decision (and which were primarily related to PSLRA).

VI. ALTERNATIVE LITIGATION-RISK PROXIES

We have used membership in high-litigation-risk industries to identify firms subject to high litigation risk. Jones and Weingram (1996) estimate a litigation prediction model that includes most of the litigation-risk proxies identified in the literature. They find that the following variables have significant explanatory power: (1) standard deviation of daily returns measured from $t - 20$ to $t - 1$ (STNDTW), where t is the date for which litigation risk is assessed; (2) skewness of returns measured from $t - 250$ to $t - 1$ (SKEW); (3) market capitalization measured on day $t - 1$ (MKTCAP); (4) cumulative turnover, estimated as the proportion of outstanding shares that traded from $t - 250$ to $t - 1$ (ECTURN); (5) cumulative return from $t - 250$ to $t - 1$ (ANRET); and (6) systematic risk, which is the Scholes-Williams adjusted beta, measured using the CRSP value-weighted index and returns

Proposition 211, and the Supreme Court decision are significantly negatively associated with LITGRISK, with t-statistics of −2.67, −1.95, and −2.24, respectively.[22] Thus, our inferences are robust to the choice of litigation-risk proxy.[23]

VII. CONCLUSIONS

On December 22, 1995, Congress enacted the Private Securities Litigation Reform Act, which increased restrictions on private litigation for securities fraud. We examine stock price reactions of firms in four high-litigation-risk industries: computers, electronics, pharmaceuticals/biotechnology, and retailing (Francis et al. 1994a). Two other studies on this issue, ST and JKN, conclude that shareholders considered PSLRA beneficial. While we find largely similar daily abnormal returns for event-related days that they examine, we present evidence that the timing of multiple confounding events makes the interpretation of these daily returns ambiguous. Results from additional analyses, beyond that conducted by ST and JKN, where significant, are largely inconsistent with their interpretation. First, results of intertemporal correlation analysis suggest that the positive abnormal return observed on 12/20/95 is more likely a response to the presidential veto and not to the House override. Second, abnormal returns for events other than the veto during the almost year-long debate on PSLRA suggest that shareholders considered PSLRA harmful. Third, we observe significantly negative cumulative abnormal returns during the last three weeks, when the final version of the bill received the congressional approval, presidential veto, and congressional override of the veto. Fourth, we find a significantly negative stock price reaction to the defeat of a California state ballot initiative, Proposition 211, intended to reverse many of PSLRA's key provisions; these abnormal returns are positively correlated with abnormal returns related to the passage of PSLRA. Finally, we find significantly negative abnormal returns to the 1994 Supreme Court decision that made it harder for shareholders to sue professional advisors suspected of willfully aiding and abetting securities violations. While the confounding timing of events makes it difficult to provide conclusive evidence, we interpret the entire body of evidence as suggesting that, on average, investors in these four high-litigation-risk industries react *negatively* to the PSLRA's restrictions on their ability to bring securities-related lawsuits.

Investors' disapproval of the PSLRA could be due to a decrease in the unexpected loss of insurance value or due to decrease in deterrence of false or misleading disclosures, which, according to Seligman (1994a), is one of the most desirable attributes of securities law. We cannot directly test the relative contributions of these two effects. Nevertheless, the litigation settlement data in Carleton et al. (1996) for the computer, electronics, and pharmaceutical /biotechnology firms provide an estimate of the maximum stock price effect of the unexpected loss of insurance value due to PSLRA.[24] The abnormal returns that we observe for these industries for some of the PSLRA events are much larger, suggesting that a decrease in deterrence due to increased restrictions on private securities litigation is likely a major factor in shareholders' disapproval of PSLRA.

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[22] We perform the following sensitivity checks. First, the observed association between our abnormal return measure and LITGRISK, which is a function of market capitalization, could be attributable to a lack of control for firm-size in estimating abnormal returns (Banz 1981). To rule out this alternative explanation we estimate the Sefcik and Thompson (1986) portfolio OLS regression models in Table 7 with market capitalization as an additional explanatory variable. The signs and significance levels of the coefficients on the litigation-risk variable, LITGRISK, remain similar to those reported in Table 7—for dependent variables PSLRAXR, P211XR, and CBDXR, t-statistics for the coefficient on LITGRISK are −2.62, −1.87, and, −2.60, respectively. Second, the rank correlation between LITGRISK and each of the return measures, PSLRAXR, P211XR, and CBDXR, is significantly negative at better than the 1 percent level. Finally, we consider cumulative abnormal returns for all PSLRA event dates with significant abnormal returns (see Table 1), instead of PSLRAXR. The portfolio OLS regression procedure estimates the coefficient on LITGRISK to be −0.49 with a t-statistic of −3.90.

[23] The Spearman correlation between abnormal returns on 12/18/95 (the veto rumor date) and LITGRISK is insignificant ($p = 0.48$), further supporting our argument that high-litigation-risk firms did not exhibit significant abnormal returns on 12/18/95.

[24] Details of the estimation may be obtained from the authors upon request.

APPENDIX

Newspaper Articles on Whether the President's Veto on the Night of December 19 Was More Surprising than the House Vote to Override the Veto on December 20

(Sources: *Wall Street Journal, Dow Jones News Service*, and newspapers covered by the Lexis-Nexis database.)

I. Articles Suggesting That the Veto Was More Surprising

1. *Wall Street Journal* 12/20/95 (Frisby and Taylor 1995b) lists the sequence of events leading to the veto, indicating that President Clinton had not made up his mind before December 19:

   12/15 (Friday)—"Mr. Clinton spent time with top class-action trial lawyers who oppose the bill...during a thank-you dinner hosted at the White House for the big donors to his campaign." (p. A3)
   12/16 and 12/17 (Saturday and Sunday)—"Mr. Clinton, a former law professor, delved into minute details of the bill over the weekend, calling law professors to get their predictions on the legislation's impact." (p. A3)
   12/18 (Monday night)—"He [Mr. Clinton] talked with Senator Dodd into the early morning hours about the pleading standards; the senator followed up yesterday by sending the president examples of cases that have been successfully litigated in states where the pleading standards are similar to the ones in the bill." (p. A3)
   12/19 (Tuesday night)—"Clinton set up a meeting late night in the Oval Office with his lawyers, aides said, to give the bill's supporters one last chance to resolve his concerns....[T]he president rejected it just hours before midnight when it automatically would have become law...." (p. A3)

2. *Wall Street Journal* 12/20/95 (Frisby and Taylor 1995b): "In the House, support is considered strong enough to override the veto with ease....The veto sets the stage for a likely override fight in the Senate." (p. A3)

3. *Dow Jones News Service* (1995a) before the House vote: "House is expected to override the veto...but Senate Republican leaders are unsure whether they will be able to collect the required two-thirds support."

4. *Dow Jones News Service* (1995b), after the House vote: "House was expected to override the veto...but the Senate vote is likely to be more contentious."

5. *Washington Post* 12/21/95 (Walsh 1995): "Clinton decided to veto the bill just hours before a midnight deadline Tuesday, surprising supporters and opponents alike...." (p. A20)

6. *Wall Street Journal* 12/21/95 (Taylor 1995): "Late Tuesday night, Mr. Clinton stunned a coalition of publicly owned companies, accountants and securities firms advocating the bill by vetoing the legislation—after indicating earlier that he planned to sign it." (p. A3)

7. *Wall Street Journal* 12/22/95 (Frisby and Taylor 1995c): "The veto was unexpected." (p. A3)

IIA. Articles Referenced by Spiess and Tkac (1997) and Johnson et al. (2001) for the News of Veto Rumor on 12/18/95 (Basis for Their Conclusion That the House Vote on 12/20/95 Was More Surprising than Was the Veto on 12/19/1995)

1. *National Journal's Congress Daily* (1995): "Speculation was rampant on Capitol Hill as congressional sources insisted the president told staff Monday he had 'tentatively decided' to veto the legislation."

2. *Washington Times* (1995): "Word has it that Mr. Clinton is reaching for the veto pen." (p. A20)

IIB. Articles Suggesting That the Market May Not Have Considered the Veto Rumor on 12/18/95 Credible

1. *National Journal's Congress Daily* (1995): "Clinton late Monday was deciding whether to sign the securities litigation reform bill, White House Press Secretary Michael McCurry said."

2. *Associated Press* 12/20/95 (Fournier 1995): "As late as Monday aides were saying they were nearly certain that Clinton would sign the bill."

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———. 1998b. House votes to restrict investor suits in state courts. 5:30 PM. (July 22).

———. 1998c. Senate clears national standard for shareholder suits. 6:42 PM. (October 13).

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BOOK REVIEWS

Jane L. Reimers, Editor

Editor's Note: The two books reviewed in this issue are completely different, but the topics of both should have wide appeal. The first, done by Lisa Koonce, is an excellent review of a book related to several areas of accounting research. The second, done by Richard Brief, should be of interest to all accountants. If you have ever doubted the appeal of accounting history, please take time to read this outstanding review of an equally outstanding book.

SCHLEIFER, ANDREI. *Inefficient Markets: An Introduction to Behavioral Finance* (Oxford: Oxford University Press, 2000, pp. 216, $19.95).

This book provides a compelling overview of the relatively new area of research of behavioral finance. It lays out the traditional assumptions behind efficient markets and then carefully uses published research to explain why those assumptions may not be valid. Although the word "behavioral" typically connotes experiments, all of the research described in the book is archival or analytic. The "behavioral" spin comes from the psychological theories that are cited to support the archival or analytical findings. Many of the chapters in this book draw on Schleifer's own research.

Inefficient Markets begins with an assessment of the strength of the three basic assumptions underlying the traditional theory of efficient markets. First, investors are rational. Second, to the extent they are not rational, their behaviors are random and therefore cancel out in the aggregate without affecting prices. Third, to the extent that investors are systematically irrational, there are rational arbitragers who eliminate their influence on prices. Schleifer concludes that these assumptions may be substantially weaker than is generally believed and lays out the research that calls them into question.

Regarding the rationality of investors, Schleifer describes a wealth of behavioral finance research showing, for example, that investors are not Bayesian and that their judgments and decisions are systematically influenced by how a problem is framed. Further, he provides evidence that these individual behaviors are systematic (i.e., not random), thereby laying the groundwork for his development of a theory of investor sentiment, which he argues is one of two essential inputs to behavioral finance theory. Schleifer discusses much of the research on under- and over-reaction to information (including some research done in accounting), and reviews the evidence suggesting that these market regularities are due to the behavioral heuristics of representativeness and conservatism.

Schleifer spends a large portion of the book discussing what he considers to be the other essential element of behavioral finance theory—limited arbitrage opportunities. Drawing on his and others' research, he makes a convincing case as to why real-world arbitrage is far from perfect. For example, he explains how the mispricing of closed-end funds is the logical consequence of arbitrage limited by noise trader risk. Unless the arbitrager has an infinite horizon and is never forced to liquidate, an arbitrager who buys a closed-end fund at a discount to net asset value while selling short its underlying portfolio (even if feasible and costless) will likely not have an effective arbitrage opportunity. Schleifer is very clear in making the case that when arbitragers need to worry about financing and maintaining positions and where price divergence can become worse before getting better, arbitrage is "rationally" limited. Schleifer also successfully uses the Long-Term Capital Management debacle as an illustration of how real-world arbitrage activity can actually exacerbate the negative effects of panics in financial markets. Overall, Schleifer nicely develops and supports his argument that effective arbitrage is limited in many real-world situations, thereby undercutting a basic assumption of the efficient market hypothesis.

Throughout the book, Schleifer alternates between formal models and more layman-like discussions. This approach will appeal to readers of various levels of comfort with formal modeling and econometrics. In addition, he provides a discussion of "open problems" at the end of the book, thereby providing readers with some important food for thought for their own research.

This book is particularly relevant to many individuals—academics with research interests in finance, financial accounting, or economics as well as investors, analysts, traders, and portfolio managers. This book could be used successfully in an M.B.A. accounting or finance classroom or in a Ph.D. seminar. Given the strong prior beliefs that many have about efficient markets, I suspect that not everyone will become "a believer" in the psychology-based behavioral perspective of financial markets after reading the book. It is clear that Schleifer's own beliefs are that markets are not efficient. However, he does a nice job of not going overboard in his statements about market inefficiency. He is careful to acknowledge that scientific proof about market efficiency is difficult to achieve.

Schleifer's book serves an important purpose. If we are to make advances in our theories of investor behavior and financial markets, then it is essential that the arguments behind different paradigms (including behavioral finance) be understood. Schleifer provides such an understanding.

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ZEFF, STEPHEN A., *Henry Rand Hatfield: Humanist, Scholar and Accounting Educator* (Stamford, CT: JAI Press Inc. 2000, pp. 515, $78.50).

The focus of this meticulously crafted book is on the life and work of Henry Rand Hatfield, but Stephen Zeff's biography also is concerned with developments in accounting thought and business education in the first four decades of the twentieth century, a time when Hatfield and others like Paton played an important role. The first four chapters chronicle Hatfield's life and work. Chapter 5, which includes a section by Basil S. Yamey, assesses Hatfield's contributions to the field of accounting and its history. Chapter 6 deals with the "personal Hatfield." This is followed by reprints of a selection of Hatfield's unpublished and published papers (with comments and notes by Zeff), remarks on the unpublished papers by Maurice Moonitz, further remarks on Hatfield by Moonitz, a list of Hatfield's publications, and a lengthy index. The book is the first volume in the series, *Studies in the Development of Accounting Thought*, edited by Gary John Previts and Robert J. Bricker.

The book opens with a Preview by Maurice Moonitz who was Hatfield's student at Berkeley, a Preface, and an outline of Hatfield's career. Hatfield was born in 1866. His father, Rev. Robert Miller Hatfield, who was imbued with the Puritan-Methodist tradition, had a deep influence on Henry, who completed a course of study in the classics at the age of 16 and continued his classical studies at Northwestern University. But after two years at Northwestern, Hatfield left to join a private banking firm and bond house in Chicago, S. A. Kean & Co. (Kean was his brother-in-law), where Hatfield was introduced to the world of finance and accounting as a bookkeeper, clerk, and later as the manager of the firm's New York office. The firm became insolvent in 1890. Allegations of fraud were made against Kean, and this was followed by a trial where Hatfield was a witness. The case was settled when "friends" arranged to pay creditors 35 cents on the dollar.

Soon after this, Hatfield returned to Northwestern where he switched his major from the classics to German, probably because of the influence on him of his brother, James, who received a Ph.D. in German and then taught German at Northwestern. He also took nine credits in political economy and, upon graduation, started work on a Ph.D. in political economy at the University of Chicago. He completed the degree in 1897, with the thesis, "Municipal Bonding in the United States," which was reported to be 1,923 pages. No copies survived. Then, after a year at Washington University in St. Louis where he taught a variety of courses including psychology, Hatfield returned to the University of Chicago and was apparently the first faculty member connected with the College of Commerce and Politics. In 1901, he taught a course with some, but not exclusive, accounting content and in the next year taught a course in the "principles" of accounting. Hatfield was on the faculty of the University of Chicago until 1904, when he accepted an appointment as associate professor of accounting at the University of California, where, except for short periods, he continued to teach until his death in 1945. Hatfield's education, business, and teaching experience is an uncommon background for the first full-time academic appointment in accounting in the United States!

Zeff conjectures that even though Hatfield never took a formal course in accounting, his experience with Kean & Co. and his dissertation research qualified him for the task of developing a course in accounting. Hatfield himself states that his specialization in accounting was an "accident" (p. 34). However, it's tempting to argue that because of the surely traumatic events that followed the insolvency of Kean & Co., Hatfield, after completing the Ph.D., chose accounting as a field of study because he recognized the important economic role that accounting played. It is also true that demand for accounting was growing and that the subject was intellectually appealing to Hatfield.

Modern Accounting was published in 1909. Zeff notes that at this time, an American literature on accounting "hardly existed" (pp. 65–66) and that the situation in Europe was not much better. While it is true that the U.S. accounting literature was sparse in 1909 (only 2 U.S. texts published before 1909 were mentioned by Zeff), the literature in the U.K. was much more extensive. Indeed, *Modern Accounting* contains many cites to the U.K. sources. Actually, it can be argued that "modern" British accounting literature predates the literature on this side of the Atlantic by 25 years and Hatfield's emphasis on case law dealing with accounting issues had been characteristic of British writers for 20–30 years prior to the publication of *Modern Accounting*.

In 1922, Hatfield presented a paper, "*De Computis et Scripturis*," to the Berkeley Club. It was revised and "An Historical Defense of Bookkeeping" was presented in 1923 to the eighth annual meeting of the American Association of University Professors (the predecessor organization of the American Accounting Association) and was a "sensation." It became one of the most celebrated, scholarly papers in accounting (with cites to classic literature, history, and mathematics). The purpose was to persuade his audience of the respectability of accounting with the following (one of many) famous lines: "The contempt for accounting is not limited to university circles, but is well-nigh universal. It is evidenced by ignorance of the subject, by condescension toward its devotees, by their exclusion from polite literature."

What motivated Hatfield to write this? Zeff suggests that it was the condescension that Hatfield himself was suffering (p.121). Accounting was rejected as an academic field of study by non accountants and accountants. Furthermore, Hatfield had a classical education, studied German, and was a political scientist. His perspective was international. He had no formal training in accounting and was not a C.P.A. He attended very few the annual meetings of the American Accounting Association and its predecessor organization. He wanted to inspire students to think "rather than teach them anything" (p. 207). In short, Hatfield was "different." He also was a tough critic and intellectually honest. Small wonder that Hatfield felt condescension, perhaps even some hostility, from others.

In 1938, Hatfield coauthored (with Thomas H. Sanders and Underhill Moore) *A Statement of Accounting Principles*, which was published by the American Institute of Accountants (now the AICPA). Zeff comments that a younger Hatfield might have "withdrawn as a coauthor of a volume with which he was so fundamentally in disagreement" (p. 178). Hatfield also had reservations about an American Accounting Association Committee report (1936), "A Tentative Statement of Accounting Principles Affecting Corporate Reports," of which he had this to say: "My first criticism is that the report begins with a criminal assault on the English language" (p. 384).

Hatfield had major interests in bookkeeping history, depreciation, and price level accounting and wrote many book reviews (54 reviews were written from 1895 to 1943). His last major academic effort was the Harvard lecture, "Surplus and Dividends" given in 1942, three years before his death. While all of Hatfield's published books and articles were on topics in accounting, many of his shorter pieces, book reviews, and unpublished papers were on a broad range of subjects.

In assessing Hatfield's contributions, Zeff points out that, despite his teaching commitment to accounting, he seemed to be ambivalent about whether accounting was an academic field and, apparently, tried to keep the size of the full-time faculty at Berkeley down. While "An Historical Defense" argued eloquently for more respect of the field, Hatfield may not have thought that accounting is a proper subject for a Ph.D. (He had one doctoral student.) Yamey also comments on another contradiction. While Hatfield was the "foremost historian of accounting of his generation," "what he could have achieved greatly exceeded what he managed to achieve...." (p. 244). Could Hatfield have explained theses paradoxes? Perhaps. He certainly questioned whether it is "better to write about things than to do them" and whether the overemphasis upon the possession of a doctor's degree as a requisite to teaching is a mistake" (p. 375).

Hatfield was fiercely independent. In "A Professor Looks at Germany" (1933), a talk given at the Berkeley Club that recounts his experiences during a visit to Germany in the first half of 1933, Hatfield was keenly aware that his views about Nazi Germany would be sharply criticized by some members of the audience. (To put this time period in historical perspective, Hitler became Chancellor on January 30, 1933. While terrible things were happening, few were forecasting the horrors that would occur. For a gripping account of the Nazi years by an academic, see the two-volume work by Victor Klemperer, *I Will Bear Witness* 1933–1941/1942–1945 (New York, NY: Random House 1998/1999).) Knowing this and surely being sympathetic to this reaction, but being unwilling to compromise, Hatfield concluded the paper with this extraordinary *Apologia* (p. 368): "... whatever I may have said, I am not a Nazi, I disbelieve in their doctrines and loathe and abhor their practices, and I have never yet, nor do I intend ever in the future, to torture any Jew."

In his 1937 retirement address to the Berkeley Club (pp. 368–383), Hatfield devotes most of the talk to a review of his academic life and provides, as Zeff says, "revealing insights into Hatfield as a person." What is somewhat surprising is that in this talk, Hatfield makes only a single (and quite sardonic) comment about his work in accounting.

> The most refreshing comment that has come to me in regard to my work is as follows: A young accountant was given a copy of my first book which, while not so planned, came to him on Christmas

Editorial Data

The following table contains information about turnaround time for manuscripts received (including revisions) in the 12-month period ended February 28, 2001. Turnaround time is the number of days between the date that the manuscript was received *and the date of the editor's letter to the author(s).*

	Number of Manuscripts	Cumulative Number	Cumulative Percent
0 ≤ Days ≤ 30	74	74	25.69
31 ≤ Days ≤ 60	101	175	60.76
61 ≤ Days ≤ 90	83	258	89.58
91 ≤ Days ≤ 120	26	284	98.61
121 ≤ Days	4	288	100.00

The mean turnaround time was 51 days; the median turnaround time was 53 days.

EDITORIAL POLICY AND STYLE IN FORMATION

EDITORIAL POLICY

According to the policies set by the Publications Committee (which were endorsed by the Executive Committee and were published in the *Accounting Education News*, June 1987), *The Accounting Review* "should be viewed as the premier journal for publishing articles reporting the results of accounting research and explaining and illustrating related research methodology. The scope of acceptable articles should embrace any research methodology and any accounting-related subject, as long as the articles meet the standards established for publication in the journal....No special sections should be necessary. The primary, but not exclusive, audience should be—as it is now—academicians, graduate students, and others interested in accounting research."

The primary criterion for publication in *The Accounting Review* is the significance of the contribution an article makes to the literature.

The efficiency and effectiveness of the editorial review process is critically dependent upon the actions of both authors submitting papers and the reviewers. Authors accept the responsibility of preparing research papers at a level suitable for evaluation by independent reviewers. Such preparation, therefore, should include subjecting the manuscript to critique by colleagues and others and revising it accordingly prior to submission. The review process is not to be used as a means of obtaining feedback at early stages of developing the research.

Reviewers and associate editors are responsible for providing critically constructive and prompt evaluations of submitted research papers based on the significance of their contribution and on the rigor of analysis and presentation. Associate editors also make editorial recommendations to the editor.

MANUSCRIPT PREPARATION AND STYLE

The Accounting Review's manuscript preparation guidelines follow (with a sight modification) the B-format of *The Chicago Manual of Style* (14th ed.; University of Chicago Press). Another helpful guide to usage and style is *The Elements of Style*, by William Strunk, Jr., and E. B. White (Macmillan). Spelling follows *Webster's International Dictionary*.

FORMAT

1. All manuscripts should be typed in 12-point font on one side of 8½ × 11" good quality paper and be double spaced, except for indented quotations.
2. Manuscripts should be as concise as the subject and research method permit, generally not to exceed 7,000 words.
3. Margins of at least one inch from top, bottom, and sides should facilitate editing and duplication.
4. To promote anonymous review, authors should not identify themselves directly or indirectly in their papers or in experimental test instruments included with the submission. Single authors should not use the editorial "we."
5. A cover page should show the title of the paper, the author's name, title and affiliation, email address, any acknowledgments, and a footnote indicating whether the author would be willing to share the data (see last paragraph in this statement).

Pagination: All pages, including tables, appendices and references, should be serially numbered. Major sections should be numbered in Roman numerals. Subsections should not be numbered.

Numbers: Spell out numbers from one to ten, except when used in tables and lists, and when used with mathematical, statistical, scientific, or technical units and quantities, such as distances, weights and measures. For example: *three days; 3 kilometers; 30 years*. All other numbers are expressed numerically.

Percentages and Decimal Fractions: In nontechnical copy use the word percent in the text.

Hyphens: Use a hyphen to join unit modifiers or to clarify usage. For example: *a well-presented analysis; re-form*. See *Webster's* for correct usage.

Keywords: The abstract is to be followed by four keywords that will assist in indexing the paper.

ABSTRACT/INTRODUCTION

An Abstract of about 100 words should be presented on a separate page immediately preceding the text. The Abstract should concisely inform the reader of the manuscript's topic, its methods, and its findings.

Keywords and the Data Availability statements should follow the Abstract. The text of the paper should start with a section labeled "I. Introduction," which provides more details about the paper's purpose, motivation, methodology, and findings. Both the Abstract and the introduction should be relatively nontechnical, yet clear enough for an informed reader to understand the manuscript's contribution. The manuscript's title, but neither the author's name nor other identification designations, should appear on the Abstract page.

TABLES AND FIGURES

The author should note the following general requirements:

1. Each table and figure (graphic) should appear on a separate page and should be placed at the end of the text. Each should bear an Arabic number and a complete title indicating the exact contents of the table or figure. Tables and figures should define each variable. The titles and definitions should be sufficiently detailed to enable the reader to interpret the tables and figures without reference to the text.
2. A reference to each graphic should be made in the text.
3. The author should indicate by marginal notation where each graphic should be inserted in the text.
4. Graphics should be reasonably interpreted without reference to the text.
5. Source lines and notes should be included as necessary.

Equations: Equations should be numbered in parentheses flush with the right-hand margin.

DOCUMENTATION

Citations: Work cited should use the "author-date system" keyed to a list of works in the reference list (see below). Authors should make an effort to include the relevant page numbers in the cited works.

1. In the text, works are cited as follows: authors' last name and date, without comma, in parentheses: for example, (Jones 1987); with two authors: (Jones and Freeman 1973); with more than two: (Jones et al. 1985); with more than one source cited together (Jones 1987; Freeman 1986); with two or more works by one author: (Jones 1985, 1987).
2. Unless confusion would result, do not use "p." or "pp." before page numbers: for example, (Jones 1987, 115).
3. When the reference list contains more than one work of an author published in the same year, the suffix a, b, etc. follows the date in the text citation: for example, (Jones 1987a) or (Jones 1987a; Freeman 1985b).
4. If an author's name is mentioned in the text, it need not be repeated in the citation; for example, "Jones (1987, 115) says...."
5. Citations to institutional works should use acronyms or short titles where practicable; for example, (AAA ASOBAT 1966); (AICPA Cohen Commission Report 1977). Where brief, the full title of an institutional work might be shown in a citation: for example, (ICAEW *The Corporate Report* 1975).
6. If the manuscript refers to statutes, legal treatises or court cases, citations acceptable in law reviews should be used.

Reference List: Every manuscript must include a list of references containing only those works cited. Each entry should contain all data necessary for unambiguous identification. With the author-date system, use the following format recommended by *The Chicago Manual*:

1. Arrange citations in alphabetical order according to surname of the first author or the name of the institution responsible for the citation.
2. Use author's initials instead of proper names.
3. Date of publication should be placed immediately after author's name.
4. Titles of journals should not be abbreviated.
5. Multiple works by the same author(s) in the same year are distinguished by letters after the date.
6. Inclusive page numbers are treated as recommended in *Chicago Manual* section 8.67.

Sample entries are as follows:

American Accounting Association, Committee on Concepts and Standards for External Financial Reports. 1977. *Statement on Accounting Theory and Theory Acceptance*. Sarasota, FL: AAA.

Demski, J. S., and D. E. M. Sappington. 1989. Hierarchical structure and responsibility accounting. *Journal of Accounting Research* 27 (Spring): 40–58.

Dye, R., B. Balachandran, and R. Magee. 1989. Contingent fees for audit firms. Working paper, Northwestern University, Evanston, IL.

Fabozzi, F., and I. Pollack, eds. 1987. *The Handbook of Fixed Income Securities*. 2nd edition. Homewood, IL: Dow Jones-Irwin.

Kahneman, D., P. Slovic, and A. Tversky, eds. 1982. *Judgment Under Uncertainty: Heuristics and Biases*. Cambridge, U.K.: Cambridge University Press.

Porcano, T. M. 1984a. Distributive justice and tax policy. *The Accounting Review* 59 (October): 619–636.

———. 1984b. The perceived effects of tax policy on corporate investment intentions. *The Journal of the American Taxation Association* 6 (Fall): 7–19.

Shaw, W. H. 1985. Empirical evidence on the market impact of the safe harbor leasing law. Ph.D. dissertation, The University of Texas at Austin.

Sherman, T. M., ed. 1984. *Conceptual Framework for Financial Accounting*. Cambridge, MA: Harvard Business School.

Footnotes: Footnotes are not used for documentation. Textual footnotes should be used only for extensions and useful excursions of information that if included in the body of the text might disrupt its continuity. Footnotes should be consecutively numbered throughout the manuscript with superscript Arabic numerals. Footnote text should be doubled-spaced and placed at the end of the article.

SUBMISSION OF MANUSCRIPTS

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Conservatism, Optimal Disclosure Policy, and the Timeliness of Financial Reports

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ABSTRACT: We develop a theory of the relation between biases in financial reporting and managers' incentives to issue timely voluntary disclosures. We find that firms with relatively more conservative accounting are less likely to make timely voluntary disclosures than firms with less conservative accounting. Therefore, price is more timely in reflecting the news of firms with less conservative accounting. Prior research has assumed that the timeliness by which news is impounded in price is uncorrelated with the nature of accounting earnings and has ascribed a concave earnings-return relation to the *accounting system* reporting bad news on a more timely basis than good news. In our theory, a concave relation is not necessarily attributable to a difference in the way the accounting system reports good vs. bad news. Rather, our prediction stems from how biases in mandatory financial reports determine *which* firms optimally choose to make voluntary preemptive disclosures and which do not. Hence, our theory provides an alternative explanation for the empirical findings and cautions against interpreting them as evidence that accounting is conservative. Finally, we identify means of empirically distinguishing between the alternative explanations.

I. INTRODUCTION

In this paper we study how the amount and type of bias in mandatory financial reporting affects the supply of voluntary disclosures. Specifically, we analyze how both liberal and conservative biases, which yield financial reports that overstate or understate (respectively) a firm's true underlying conditions, affect the usefulness of mandatory financial

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reports in disciplining managers' voluntary disclosures. First we show that the more conservative a firm's financial-reporting system, the lower the risk-sharing benefits of more timely disclosure and hence, the less valuable preemptive voluntary disclosures are to the firm. We then show how the connection between accounting bias and voluntary disclosure results in a concave relation between accounting earnings and contemporaneous returns. Prior empirical research has assumed that the timeliness of returns in impounding news is uncorrelated with the resulting accounting earnings, and has ascribed the concave earnings-return relation to the *accounting system* reporting bad news on a more timely basis than good news (e.g., Basu 1997). We explicitly model the timeliness of prices in reflecting information and our theory predicts a concave earnings-return relation whether or not accounting earnings report bad news on a more timely basis than good news.

Our main goal in this study is to emphasize the need to establish theoretical links between the properties of mandatory financial reports and the amount of information managers provide through voluntary disclosures. Modeling the link between mandatory and voluntary reporting is important because empirical studies attempting to document the economic effects of changes in financial reporting generally ignore the indirect effects of such changes on other sources of information, such as voluntary disclosures. As Antle et al. (1994) show, omitting this (potentially) correlated variable makes it difficult to interpret those studies' results. In their model, it is impossible to meaningfully interpret a market reaction to an accounting disclosure without knowing the history of all nonaccounting information. Without explicit models of how changes in financial reporting affect other forms of disclosure, the empirical researcher has little guidance for overcoming this problem.

Surprisingly little theoretical guidance exists as to how changing the properties of financial statements might change other sources of information. Disclosure theories typically view financial reporting as the unbiased realization of some underlying economic variable, and rarely allow for the endogenous creation or dissemination of other information (see Diamond [1985], Indjejikian [1991], and Kim and Verrecchia [1991, 1997] for exceptions to the latter restriction). As a result, most of these studies focus only on the direct price and volume reactions to changes in the precision of what has been modeled as the market's only public information, the financial report itself.

The lack of prior research on the relation between mandatory and voluntary disclosures stems from a view that mandatory financial reporting is the primary source of information to capital markets. This view suggests that the economically interesting measure is the market's reaction to changes in the financial report itself, and that the potential indirect effects of such changes on other sources of information are at best second-order. Arguably, however, a mandatory financial-reporting regime based on completed transactions may be better characterized as a source of confirmatory information than as a primary source of timely (forward-looking) information (e.g., Ball and Brown 1968). Within this alternative view the *indirect* informational effects are by definition the first-order effects (Gigler and Hemmer 1998; Dye 1998). Our analysis does not pre-assign either of these two roles (primary or confirmatory) to the financial-reporting system. Rather, we analyze the relation between accounting numbers and prices using a model rich enough to endogenously determine the financial reports' specific informational role.

Our final motivation for this study stems from the recent interest in assessing the degree of conservatism in financial statements based on observed relations between price reactions and earnings. Basu (1997) primarily initiated this approach, finding that, in a regression of earnings on returns, the relation is stronger for negative than for positive returns. Basu (1997) interprets this as evidence that financial statements are, on average, a more timely source of bad news than they are of good news and, in turn, that financial statements are conservative. A number of subsequent papers have employed Basu's (1997) approach to order both financial reports and financial-reporting regimes according to the degree of conservatism and/or timeliness (e.g., Ball et al. [2000] rank different national reporting regimes by degree of conservatism, and Bushman et al. [1999] rank financial statements within a reporting regime by timeliness).

Our study of how properties of financial reports affect managers' incentives to make credible voluntary disclosures centers on a principal-agent model akin to the "communication of post decision information" model Dye (1983) analyzes. That is, we model a setting where shareholders write a contract that gives a manager the incentives not only to "work hard," but also, when optimal, to obtain and voluntarily disclose information about firm performance before a mandatory financial report becomes available.

Our approach to modeling voluntary disclosures may seem unorthodox, but this departure from convention overcomes several limitations of traditional disclosure models. First, standard disclosure models allow managers discretion only with respect to whether to disclose—but not what to disclose. In particular, these models assume that if a manager makes a voluntary disclosure, he discloses this information completely and truthfully. This implies that neither mandatory financial reports nor any other mechanisms play a role in ensuring the truthfulness or credibility of the voluntary disclosures. Second, traditional models of voluntary disclosure effectively treat a manager's disclosure incentives as exogenous, and rely on the existence of proprietary costs (incurred as a result of making private information public) to prevent full disclosure. However, models that rely on proprietary costs assume that shareholders cannot contract directly with the manager, because the ensuing (optimal) contracts would ensure that the manager does not disclose his private information in order to avoid the unnecessary proprietary costs. So in effect, traditional disclosure models that assume (1) that the manager always reports truthfully, and (2) that shareholders cannot directly contract with the manager, provide insights into disclosures that market participants would ideally (and optimally) write contracts to avoid.

Our primary objective is to identify how less conservative financial statements induce managers to make truthful disclosures. This study avoids the limitations of the traditional disclosure literature by explicitly modeling the shareholder-manager relationship. When shareholders can contract with the manager to address a shareholder-manager conflict, the optimal contract provides the manager with explicit incentives to make truthful voluntary disclosures. Thus, our conclusions result from the equilibrium relation between mandatory reporting and truthful voluntary disclosure, rather than from an exogenously imposed truth-telling assumption.

Neither does our model rely on the existence of (exogenously imposed) proprietary costs to explain managers' disclosure behavior. In this model, incentives for truthful voluntary disclosures are determined optimally, so managers engage in voluntary disclosure only when it leads to an *ex ante* Pareto improvement. Moreover, the issue of whether managers should make these disclosures publicly to capital markets, or privately, as internal communications to owners, is moot. In the absence of exogenously imposed proprietary costs, managers and shareholders would be indifferent between public and private communication, and there is no (endogenous) reason that the manager could not or would not make the communication public. Of course, benefits to better-informed share prices (e.g., increased market liquidity, lower cost of capital) suggest there is a strict value to making the disclosure public when the disclosure is optimal for contracting purposes. In our view, then, the absence of exogenously imposed proprietary costs together with endogenous incentives for truthful reporting make our model better suited than traditional disclosure models to explain voluntary disclosure behavior, such as issuing earnings forecasts, that the market rewards and that have little or no proprietary cost.

Our paper proceeds as follows. Section II presents our model. In Section III, we establish the relation between the nature of bias in the reporting entity's accounting system and the optimal disclosure policy determined by the shareholders. Section IV demonstrates that, although the pattern of the earnings-returns relation implied by our model is identical to that documented by Basu (1997), this relation arises even when the financial-reporting regime does not have a conservative bias. Section V provides a brief summary.

II. MODEL

Consider a setting in which risk-neutral shareholders hire a risk- and effort-averse manager to supply unobservable effort. The manager's actions increase the likelihood of a desirable future event, such as improvements in a production process, improved standing of the company brand, development of a new product, and gaining access to new markets. These events result in subsequent transactions that the accounting system first records and then, at a predetermined time, aggregates and releases to the public as a part of the company's financial report.

In the interest of simplicity and transparency, we restrict our attention to binary actions and outcomes. We limit the manager's effort choice to be either high or low so that $e \in \{e^h, e^l\}$. The manager's effort increases the probability of achieving output of higher economic value. The realized output is denominated in true economic income, which only the manager observes. We denote this output, x, and restrict our attention to a binary setting where its value can be either high or low. Thus we have $x \in \{x^H, x^L\}$, where $x^H > x^L$.[1] The expected effect of the manager's effort on the value of output is determined by $pr(x|e)$, where:

$$1 > pr(x^H|e^h) > pr(x^H|e^l) > 0. \qquad (1)$$

To economize on notation, hereafter we denote $pr(x^H|e^h)$ by P^h and $pr(x^H|e^l)$ by P^l. We assume that $(x^H - x^L) \times (P^h - P^l)$ is sufficiently large that it is always optimal to induce the high effort.

We now introduce the properties of the firm's mandatory financial reports. Our analysis assumes that accounting recognition criteria are such that accounting earnings over a given sub-period (here, the contracting horizon) may not accurately reflect the firm's true economic position. For example, the manager could enter into a contract that increases the firm's expected future cash flows. However, if the contract remains mutually unexecuted at the end of the reporting period, specific revenue recognition criteria are not met and the firm does not recognize the increase in economic income as accounting income. Specific recognition criteria combined with the nature of the production function can lead to either conservative- or liberal-biased accounting earnings for a given period (i.e., either an understatement or an overstatement of true economic earnings, respectively). Moreover, since it is often not known until the end of the reporting period whether a firm will meet a specific criterion, the resulting bias depends partly on random events.

Our aim here is to capture, in the simplest possible way, the randomness and the potential for bias introduced via specific criteria for when (and how) to represent economic events in a company's mandatory financial reports. Accordingly, we assume that accounting earnings can also be either high or low, and represent the financial-statement information about x by $y \in \{y^H, y^L\}$, where $y^H > y^L$. Our one-period model captures the properties of the firm's financial-reporting system by the probability that high accounting earnings result when true economic income is high, λ^H, and that low accounting earnings will result when true economic income is indeed low, λ^L. We require that $\lambda^H \geq 1 - \lambda^H$ and that $\lambda^L \geq 1 - \lambda^L$ so that "high" and "low" accounting earnings retain their literal interpretation.

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[1] We label choice variables with lowercase superscripts and random variables with uppercase superscripts.

This structure allows us to classify accounting systems as liberal, conservative, or unbiased based on the relation between the *ex ante* expected value of x, the true economic income, and the expected accounting income, y. Specifically, in the case of an unbiased accounting system we have:

$$\text{Prob}(y^H)y^H + \text{Prob}(y^L)y^L = P^h x^H + (1 - P^h)x^L, \tag{2}$$

while for a conservative accounting system the left-hand side of equation (2) is smaller than the right-hand side and for a liberal system the left-hand side is greater than the right-hand side. With the structure of the uncertainty introduced above, the left-hand side of equation (2) can be rewritten as:

$$(P^h\lambda^H + (1 - P^h)(1 - \lambda^L))y^H + (P^h(1 - \lambda^H) + (1 - P^h)\lambda^L)y^L,$$

which is increasing in λ^H and decreasing in λ^L. Note that the probability that accounting earnings overstates economic earnings is $1 - \lambda^L$ and the probability that accounting earnings understates economic earnings is $1 - \lambda^H$. Hence, an accounting system is less conservative if, *ceteris paribus*, λ^L is smaller or λ^H is larger. Conversely, the accounting system is more conservative if λ^L is larger or λ^H is smaller. Figure 1 summarizes the relations between e and x and between x and y.

Because y is a noisier indicator of e than is x, the variable most useful for inducing the manager to exert high effort is x. This follows from Holmström (1979). However, because only the manager observes x directly, the best the shareholders can do is to rely on the manager's communication of x, denoted $\hat{x}$. Therefore, $\hat{x}$ can be thought of as a manager's *voluntary* disclosure. To exploit this information in the contract the shareholders must get the manager to communicate it *before* the verifiable signal, y, becomes available, because uncertainty about the realization of y is needed to induce the manager's truthful communication of x. Since y represents the accounting earnings for the contracting period, we interpret the report, $\hat{x}$, issued before y becomes known, as voluntarily issuing an earnings forecast.

We assume that writing and adopting a *communication contract* introduces exogenously determined costs that shareholders do not incur with a *no-communication* contract. Communication contracts are more costly to implement because they require the manager to collect and disseminate information at an earlier time. This may in turn require additional organizational activities, such as engaging in a budgeting process. $C > 0$ represents the incremental cost of making x available to the manager before the end of the contracting horizon and thus the cost of making $\hat{x}$ available for contracting. Since delaying the production of information carries its own cost, providing the manager with a means for obtaining and communicating x early will be desirable only when the net benefit from using this piece of information in the contract exceeds the cost.[2]

Finally, we assume that the two parties to the contract are of the standard type. That is, the shareholders are risk-neutral with preferences specified over expected consumption opportunities. The risk-averse manager's utility function is additively separable in utility

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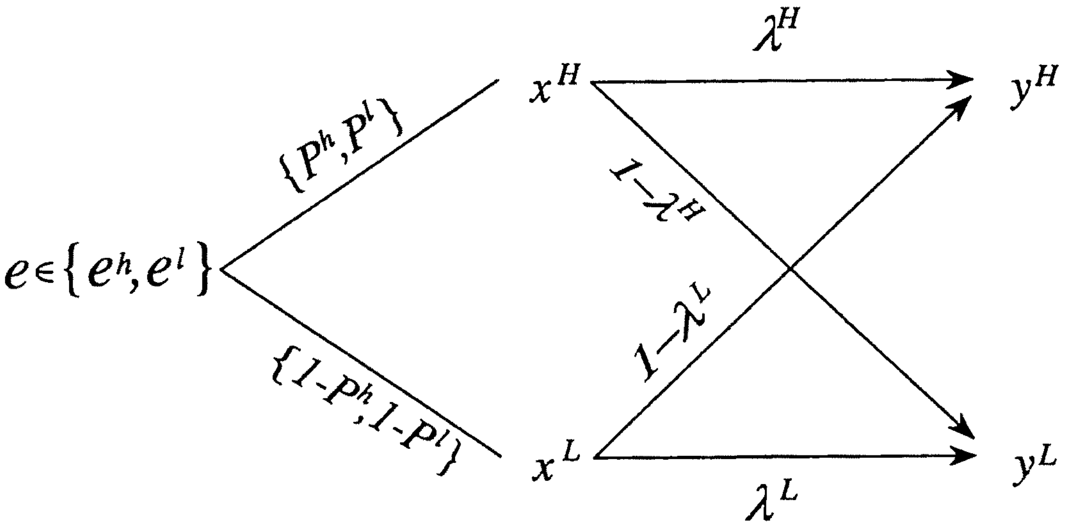
[2] For sufficient conditions when it is optimal to have a manager share post-decision information when there are no exogenous costs to communication, see Dye (1983).

FIGURE 1
Probabilistic Relation between Effort and Feasible Economic and Accounting Outcomes



e = productive effort;
x^H = a High economic value realization;
x^L = a Low economic value realization;
y^H = a High earnings realization;
y^L = a Low earnings realization;
P^h = the probability for x^H if the manager supplies a high level of effort;
P^l = the probability for x^H if the manager supplies a low level of effort;
λ^H = the probability that x^H is followed by y^H; and
λ^L = the probability that x^L is followed by y^L.

for consumption opportunities provided by compensation received at the end of the contracting horizon and the effort exerted over the contracting horizon. Letting $s(\cdot)$ denote the manager's compensation, which can be a function of all pieces of information to which *both* parties have access, the manager's utility function is given by:

$$H(s(\cdot), e) = U(s(\cdot)) - V(e).$$

$U'(\cdot) > 0$ and $U''(\cdot) < 0$ by risk aversion, and $V'(\cdot) > 0$ by effort aversion. Without loss of generality we will normalize $V(e^l) = 0$ and $V(e^h) = v$. Finally, $G(\cdot)$ represents the inverse of the manager's utility for compensation, so that $s(\cdot) \equiv G(U(s(\cdot)))$.

III. ANALYSIS

The shareholders' key problem is to choose the type of contract to offer the manager. They can choose either to provide the manager with a contract that gives him a financial incentive to truthfully report his private information before he learns the exact earnings realization, or they can offer a contract that does not encourage the manager to engage in this type of preemptive voluntary disclosure. We start our investigation by specifying the two programs that, when solved, respectively yield (1) the best contract without early disclosure, and (2) the best contract that provides the manager with access to private information, x, and induces him to reveal it as soon as he learns it. The contractual benefit of requiring early disclosure of x is simply the difference between the expected payments of these two contracts. Shareholders compare this amount to the cost of enabling early disclosure, C, to determine the type of contract to offer the manager.

When shareholders do not want the manager to make timely disclosures, the optimal use of the financial reports to encourage the manager to choose the high effort level is the solution to the following "no-communication" (NC) program:

$$\min_{s(y)} P^h[\lambda^H s(y^H) + (1-\lambda^H)s(y^L)] + (1-P^h)[\lambda^L s(y^L) + (1-\lambda^L)s(y^H)] \tag{NC}$$

s.t.

$$P^h[\lambda^H U(s(y^H)) + (1-\lambda^H)U(s(y^L))] + (1-P^h)[\lambda^L U(s(y^L)) + (1-\lambda^L)U(s(y^H))] - v \geq \underline{U} \tag{IR}$$

$$\begin{aligned} &P^h[\lambda^H U(s(y^H)) + (1-\lambda^H)U(s(y^L))] + (1-P^h)[\lambda^L U(s(y^L)) \\ &\quad + (1-\lambda^L)U(s(y^H))] - v \geq P^l[\lambda^H U(s(y^H)) + (1-\lambda^H)U(s(y^L))] \\ &\quad + (1-P^l)[\lambda^L U(s(y^L)) + (1-\lambda^L)U(s(y^H))] \end{aligned} \tag{IC}$$

The shareholders' objective in NC is to minimize the expected compensation to the manager given: (1) that the contract is acceptable to the manager, IR (individual rationality), and (2) that it provides incentives for the manager to work hard, IC (incentive compatibility). The solution to this program appears in Lemma 1.

Lemma 1: The optimal no-communication contract is given by:

$$\begin{aligned} U(s(y^H)) &= \underline{U} + v - \frac{vP^h}{P^h - P^l} + \frac{v\lambda^L}{(P^h - P^l)(\lambda^H + \lambda^L - 1)} \\ U(s(y^L)) &= \underline{U} + v - \frac{vP^h}{P^h - P^l} - \frac{v(1-\lambda^L)}{(P^h - P^l)(\lambda^H + \lambda^L - 1)}. \end{aligned}$$

On the other hand, if the shareholders want to provide the manager with an incentive to reveal x truthfully before the realization of y, then the optimal contract that also ensures that the manager chooses the high effort level is the solution to the following "truthful communication" (TC) program:

$$\min_{s(x,y)} P^h[\lambda^H s(x^H, y^H) + (1-\lambda^H)s(x^H, y^L)] + (1-P^h)[\lambda^L s(x^L, y^L) + (1-\lambda^L)s(x^L, y^H)] \tag{TC}$$

s.t.

$$\begin{aligned} &P^h[\lambda^H U(s(x^H, y^H)) + (1-\lambda^H)U(s(x^H, y^L))] + (1-P^h)[\lambda^L U(s(x^L, y^L)) \\ &\quad + (1-\lambda^L)U(s(x^L, y^h))] - v \geq \underline{U} \end{aligned} \tag{IR}$$

$$\begin{aligned} &P^h[\lambda^H U(s(x^H, y^H)) + (1-\lambda^H)U(s(x^H, y^L))] + (1-P^h)[\lambda^L U(s(x^L, y^L)) \\ &\quad + (1-\lambda^L)U(s(x^L, y^H))] - v \geq P^L[\lambda^H U(s(x^H, y^H)) + (1-\lambda^H)U(s(x^H, y^L))] \\ &\quad + (1-P^L)[\lambda^L U(s(x^L, y^L)) + (1-\lambda^L)U(s(x^L, y^H))] \end{aligned} \tag{IC}$$

$$\lambda^H U(s(\hat{x}^H, y^H)) + (1 - \lambda^H)U(s(\hat{x}^H, y^L)) \geq \lambda^H U(s(\hat{x}^L, y^H)) + (1 - \lambda^H)U(s(\hat{x}^L, y^L)) \quad (TT^H)$$

$$\lambda^L U(s(\hat{x}^L, y^L)) + (1 - \lambda^L)U(s(\hat{x}^L, y^H)) \geq \lambda^L U(s(\hat{x}^H, y^L)) + (1 - \lambda^L)U(s(\hat{x}^H, y^H)) \quad (TT^L)$$

Program TC differs from program NC in several respects. First, the manager's communication of x becomes available for contracting. In a world where both the manager and the shareholders could observe x directly, we would not use accounting earnings, y, in the contract at all because x is a sufficient statistic for y with respect to e. But when only the manager observes x directly, we must keep accounting earnings, the only hard evidence on the manager's information, in the contract in order to induce the manager to tell the truth about his observation of x. Thus, TC has two additional "truth-telling" constraints, designed to ensure that the manager is always better off telling the truth than lying: TT^H when the manager observes x^H and TT^L when the manager observes x^L. The solution to program TC appears in Lemma 2 (see also Berg et al. 1990).

Lemma 2: The optimal truthful communication contract is given by:

$$U(s(\hat{x}^H, y^H)) = \underline{U} + v - \frac{vP^h}{P^h - P^l} + \frac{v\lambda^L}{(P^h - P^l)(\lambda^H + \lambda^L - 1)}$$

$$U(s(\hat{x}^H, y^L)) = \underline{U} + v - \frac{vP^h}{P^h - P^l} - \frac{v(1 - \lambda^L)}{(P^h - P^l)(\lambda^H + \lambda^L - 1)}$$

$$U(s(\hat{x}^L, y^H)) = U(s(\hat{x}^L, y^L)) = \underline{U} + v - \frac{vP^h}{P^h - P^l}.$$

Lemmas 1 and 2 illustrate some of the interesting properties of the optimal truthful communication contract and allow us to calculate the contractual benefit of early disclosure. First, note that when the manager announces that he has observed x^H, he receives the same contract as he would have under the no-communication contract. That is, conditional on observing and reporting x^H, the manager receives $s(y^H)$ if accounting earnings are high and $s(y^L)$ if they turn out to be low. Another important property of the optimal communication contract is that, when the manager reports x^L, he receives $s(x^L)$ (the conditional payment under the optimal contract when x is *directly* observable) regardless of the realization of y. Hence, the benefit of the optimal communication contract over the no-communication contract is that, when economic earnings are low, shareholders compensate the manager as though economic earnings were directly verifiable. Thus, the contractual benefit of early disclosure takes on the simple form given in Proposition 1.

Proposition 1: The expected reduction in compensation cost of using the communication contract instead of the no-communications contract is:

$$\Delta S = (1 - P^h)\{E[G(U(s(y)))|x^L] - G(E[U(s(y))|x^L])\} \geq 0.$$

Proposition 1 shows that there is always a weak contractual benefit of early disclosure. This is because, when $\hat{x} = x^L$, the communication contract pays the manager an amount that does not depend on the subsequent realization of y and yet provides the manager with the exact same level of (expected) utility as the risky no-communication contract does when x^L is realized. The inequality in Proposition 1 then follows from the convexity of the risk-averse manager's inverse utility function, $G(\cdot)$.

Based on Lemmas 1 and 2 and Proposition 1, we now address the main issue of our analysis. We focus specifically on how the amount and type of potential bias in the financial-reporting system affect the contractual benefit of having access to $\hat{x}$ and, therefore, the equilibrium amount of information publicly available at a given time. Recall that the reporting system is characterized by the probability that high economic earnings result in high accounting earnings, λ^H, and that low economic earnings will produce low accounting earnings, λ^L. A perfect accounting system (i.e., one that produces no errors) would necessarily have the property that $\lambda^H = \lambda^L = 1$. Bias is present in any system for which λ^H and λ^L are not both equal to 1. Proposition 2 provides a a useful first step toward understanding the relation between bias in a firm's financial-reporting system and its optimal disclosure policy.

Proposition 2: The expected cost of the no-communication contract is strictly higher than the expected cost of the communication contract if and only if $\lambda^L < 1$. If $\lambda^L = 1$, then the expected costs of the two contracts are identical.

To see why it is necessary to have some error in classifying low economic earnings in order to create a value of communication, recall that the benefit of having x communicated early arises from shielding the manager from the risk imposed by noise in the accounting system whenever x^L is realized. However, when $\lambda^L = 1$, the accounting system imposes no additional risk when $x = x^L$ because x^L always leads to y^L. The strict contractual value of communication when $\lambda^L < 1$ follows from the same argument. When $\lambda^L < 1$, x^L is *not* always followed by y^L. Accordingly, when $\lambda^L < 1$, the optimal no-communication contract exposes the manager to risk when $x = x^L$, but the optimal communication contract does not.

The key implication of Proposition 2 is that with $C > 0$ it is never optimal for the shareholders to use the communication contract if the accounting system has $\lambda^L = 1$, even if $\lambda^H < 1$. In contrast, whenever there is some potential that the accounting system will represent a low outcome as high, there exist values of $C > 0$ for which the shareholders would indeed choose to use the communication contract. This is true for all values of λ^H, including when $\lambda^L < \lambda^H = 1$.

Next, we turn to a more general analysis of the effect of reporting bias on the optimal voluntary disclosure policy. Specifically, we focus on the implications for the optimal voluntary disclosure policy of continuously changing the bias in the financial report, through changing λ^L and λ^H. Changing λ^L and λ^H may affect not only the amount of bias, but also the level of risk that financial reports introduce into the principal-agent relation by changing the amount of noise in the financial-reporting system. Both properties have implications for the value of timely communication. However, since we are interested in isolating the role of accounting *bias* in facilitating communication, we focus on changes in λ^L and λ^H that do not change the level of risk.

How *does* one change bias without changing risk? Risk is the property of a gamble that a so-called "risk-averse" individual will pay to avoid. One cannot, in general, assess the risk associated with a gamble simply from the properties of the gamble itself, because the notion of risk is individual-specific. However, we can quantify the amount a risk-averse individual requires to accept a gamble, the risk premium. Equivalently, for any individual, we can rank all gambles by the difference between the gamble's expected payoff and the value the individual places on the gamble, its certainty equivalent. This means that in an agency setting such as ours where it is always optimal to induce the same level of effort, changing the bias in the accounting system while keeping the level of risk constant is equivalent to changing λ^L and λ^H such that the expected compensation cost, which consists of compensation for effort and the risk premium the manager requires, remains constant.

Lemma 3 summarizes the relation between λ^L and λ^H required to ensure that the risk premium paid to the manager in the benchmark no-communication case remains constant.

Lemma 3: For any risk-averse manager, holding the amount of risk in the optimal no-communication contract constant while changing λ^L defines λ^H as an implicit function of λ^L that satisfies $\lambda^H/(\lambda^L - 1) < d\lambda^H/d\lambda^L < (\lambda^H - 1)/\lambda^L < 0$.

The technical result provided by Lemma 3 is useful for at least two reasons. First, it establishes that when comparing the bias in different accounting systems that are equally "risky" in the no-communication case, the degree of conservative bias is unambiguously increasing in λ^L. In technical terms, Lemma 3 provides a complete ordering on the set of accounting systems with respect to the degree of conservatism when the amount of risk in the benchmark setting is held constant, even though "conservatism" is fundamentally two-dimensional in our setting ($\lambda^L \times \lambda^H$). This follows because keeping the level of risk constant in the benchmark setting requires that an increase in λ^L is *always* accompanied by a decrease in λ^H. Since $1 - \lambda^L$ and $1 - \lambda^H$ represent the probabilities of an overstatement and an understatement, respectively, increasing λ^L while keeping the level of risk constant makes the accounting system *both* less likely to overstate and, simultaneously more likely to understate the true state of nature. Second, the relation detailed in Lemma 3 is exclusively a property of the accounting system, not of the manager's utility function. These features allow us to state Proposition 3, our main result, in the following general terms:

Proposition 3: Holding the equilibrium risk premium in the no-communication contract constant, $d\Delta S/d\lambda^L < 0$. Thus, the value of communication is strictly decreasing in the degree of conservatism of the financial-reporting system.

Proposition 3 confirms what Proposition 2 only hints at. *Ceteris paribus*, the more conservative the accounting system, the less the contractual value of early disclosure. This is because, as can be verified from the proof of Proposition 3, increasing λ^L while keeping the level of risk constant in the benchmark no-communication setting decreases the risk-sharing benefit to timely disclosures. As this benefit shrinks it will eventually be swamped by the cost of producing the early disclosure, C. So only firms for which the accounting standards are sufficiently liberal (i.e., λ^L is sufficiently small) would adopt a policy of providing timely voluntary disclosures.

The economic intuition behind Proposition 3 follows. The benefit of communication arises from shareholders' ability to evaluate the manager based on a more precise performance measure (x rather than y). However, because the contract must give the manager an incentive not to overstate his private information (i.e., claim $\hat{x}^H$ when he has observed x^L), the optimal communication contract relies on the subsequent earnings realization for verification (only) when the manager discloses $\hat{x}^H$. That is, the risk the imprecision in the financial report imposes on the manager in the no-communication setting is eliminated only when the manager claims $\hat{x}^L$. Since the noise by which the accounting system represents x^L is a decreasing function of λ^L, the potential for the manager's voluntary disclosure to reduce risk is also a decreasing function of λ^L. As a result, the gain to communication decreases as the likelihood that the accounting system mistakenly represents a low outcome as a high one decreases.

Figure 2 provides more formal and complete insights into the result summarized as Proposition 3. Figure 2 portrays the mapping of two optimal no-communication contracts into utility for a specific manager. The two optimal contracts are generated from two different accounting systems that result in the same expected wage. The points $\mathcal{C}^L$ and $\mathcal{C}^H$ ($\mathcal{L}^L$ and $\mathcal{L}^H$) represent the contractual payments to the manager associated with y^L and y^H, respectively, for the more conservative (liberal) accounting system.[3] Since the shareholders always design the manager's compensation contract to induce the high level of effort, holding the equilibrium risk premium constant is assured if expected compensation cost is the same under the two accounting systems. Furthermore, because both contracts are optimal, they also provide the same expected utility. Graphically, this means that the solid lines connecting $\mathcal{C}^L$ with $\mathcal{C}^H$ and $\mathcal{L}^L$ with $\mathcal{L}^H$ must intersect on the dashed horizontal line representing the manager's required expected utility from compensation, $\underline{U} + v$. The horizontal distance between the point FB and the point of intersection, SB_{NC}, is the risk premium.[4]

Recall that if the parties use a communication contract, then the optimal contract when x^H is realized is identical to the optimal no-communication contract. Therefore, the points $\mathcal{C}^L$ and $\mathcal{C}^H$ ($\mathcal{L}^L$ and $\mathcal{L}^H$) in Figure 2 also represent the optimal communication contract payments associated with y^L and y^H for the more conservative (liberal) accounting system conditional on the manager's reporting $\hat{x}^H$. By Lemma 2, $U(s(\hat{x}^L, y))$ is constant in y and independent of λ^L and λ^H. Thus, when the manager reports $\hat{x}^L$ under a communication contract, he receives the compensation/utility combination indicated by the point $\hat{x}^L$ under *either* accounting system. The amount of compensation the manager receives when he reports $\hat{x}^L$ is identified by I on the horizontal axis.

One can verify from Lemma 1 that $E[U(s(y))|x^L] = \underline{U} + v - vP^h/(P^h - P^l)$ and is therefore equal to $U(s(\hat{x}^L, y))$. Therefore, the manager's expected utility conditional on x^L is independent of both the accounting system and whether the contract uses communication. Under a no-communication contract, this level of expected utility is a convex combination of the levels of utility associated with $\mathcal{C}^L$ and $\mathcal{C}^H$ ($\mathcal{L}^L$ and $\mathcal{L}^H$) for the more conservative (liberal) accounting system. Moreover, because the expected utility conditional on x^L under an optimal communication contract is the same as that under an optimal no-communication contract, the expected *compensation* conditional on x^L for each of the two accounting systems corresponds to the point where the solid lines connecting $\mathcal{C}^L$ with $\mathcal{C}^H$ and $\mathcal{L}^L$ with $\mathcal{L}^H$ intersect with the dashed horizontal line identifying $E[U(s(y))|x^L] = U(s(\hat{x}^L, y)) = \underline{U} + v - vP^h/(P^h - P^l)$. For the more conservative (liberal) accounting system, the expected compensation conditional on x^L is identified by II (III) on the horizontal axis.

Because the benefit of communication arises from eliminating the risk in the manager's contract when x^L is realized, we calculate the *ex ante* value of communication directly as $(1 - P^h)[II - I] - C$ in the more conservative case, and as $(1 - P^h)[III - I] - C$ in the more liberal case. The terms in square brackets are the expected compensation cost savings from improved risk sharing when x^L is realized and $1 - P^h$ is the probability of that realization. That $[III - I]$ is strictly greater than $[II - I]$ reflects the result established in Proposition 3. The shareholders' choice of a disclosure policy depends on a comparison of the magnitude of the costs and benefits of early disclosures. The more liberal the financial-reporting system, the more likely this comparison favors early disclosure.

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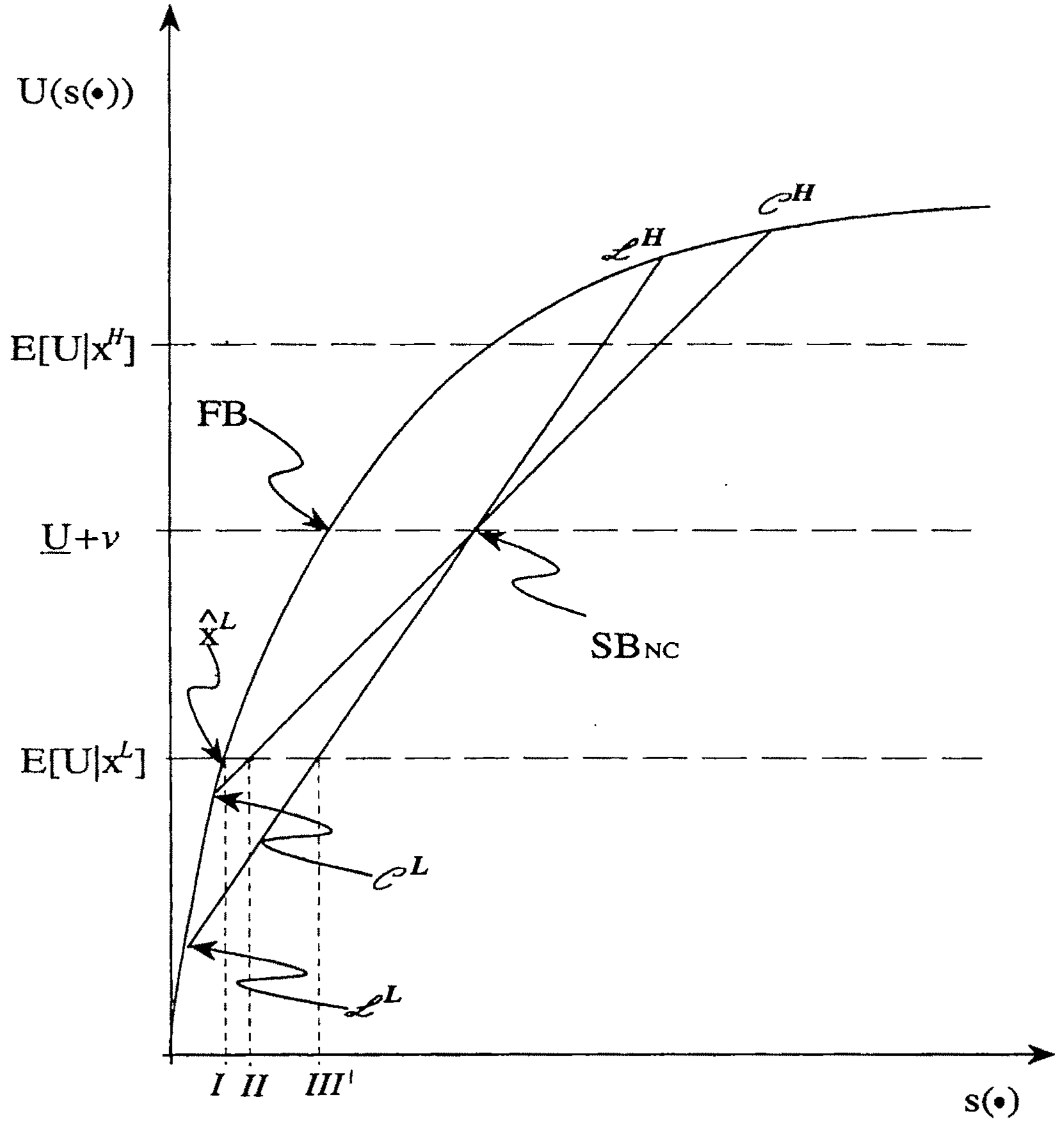
[3] As can be verified from the proof of Proposition 3, both $s(y^H)$ and $s(y^L)$ are increasing in the degree of conservatism. Thus $\mathcal{C}^L > \mathcal{L}^L$ and $\mathcal{C}^H > \mathcal{L}^H$ in Figure 2 along both dimensions.

[4] FB stands for "first-best" and SB_{NC} for "second-best, no-communication."

FIGURE 2
The Effect of Communication on Equilibrium Compensation Costs



$\underline{U} + v$ = the manager's minimum utility requirement from compensation;
$E[U|x^H]$ = the manager's expected utility conditional on observing x^H;
$E[U|x^L]$ = the manager's expected utility conditional on observing x^L;
FB identifies the manager's first-best utility/compensation;
SB_{NC} identifies the manager's second-best expected utility/compensation in the benchmark no-communication setting;
$\mathcal{L}^H$, $\mathcal{L}^L$ = utility/compensation combinations associated with y^H and y^L, respectively, for a relatively liberal accounting system;
$\mathcal{C}^H$, $\mathcal{C}^L$ = utility/compensation combinations associated with y^H and y^L, respectively, for a relatively conservative accounting system;
$\hat{x}^L$ = the utility/compensation combinations for a communication contract when the manager reports $\hat{x}^L$ under either accounting system; and
$s(\cdot)$ = compensation paid to the manager as a function of the available information.

Proposition 3 assumes that the properties of a firm's financial-reporting system result from laws and standards and that, unlike the voluntary disclosures, they are not subject to the individual firm's discretion. However, while standards and laws restrict discretion, firms typically have some ability to affect the conservatism of their financial-reporting systems. Because Proposition 3 holds constant the agency cost in the no-communication contract, it also reveals how firms would use limited discretion in choosing the bias in their accounting systems. Specifically, since the value of communication, and therefore the agency cost associated with a communication contract, is decreasing in the degree of conservatism in the financial-reporting system, firms would choose as liberal an accounting system as possible. However, firms that cannot pass the threshold over which communication becomes optimal are *indifferent* to the nature of the bias in their financial-reporting systems. Accordingly, the result that disclosing firms have less conservative financial-reporting systems also holds when firms have limited discretion to determine the properties of their financial-reporting systems.

IV. IMPLICATIONS FOR THE EARNINGS-RETURNS RELATION

We now consider how an explicit theory of the relation between conservatism in financial statements and equilibrium voluntary disclosure policy suggests an alternative interpretation of evidence reported by prior empirical research. We also show how our theory can help researchers design studies to distinguish between the two alternative plausible explanations for these results. To illustrate this point, we focus on Basu's (1997) use of the earnings-return relation to draw inferences about the degree of conservatism in accounting earnings.

Basu's (1997) maintained assumptions are that a conservative accounting system reports bad news on a more timely basis than it reports good news, whereas stock price reflects both good and bad news on a timely basis. Given these assumptions, Basu (1997) expects the slope coefficient (as well as R^2) in a regression with earnings as the dependent variable and stock returns as the independent variable to be higher for the bad news (negative returns) subsample than for the good news (positive returns) subsample. The prediction relies critically on and is a direct implication of the definition of a conservative accounting system as one that measures low economic earnings more accurately than it measures high economic earnings. In terms of our notation, this notion of conservatism is that the value of λ^L is greater than the value of λ^H. The relation between negative returns and earnings is strong because the accounting accurately reflects the bad news that caused the negative return. The relation between positive returns and earnings is weaker because the period's accounting does not capture the good news that generated the positive return as accurately. Basu (1997) and subsequent studies relying on his approach (such as Ball et al. 2000; Bushman et al. 1999), document empirical evidence consistent with this expectation.

Basu's (1997) predictions on the slope coefficients also rely critically on his second maintained hypothesis, that the timeliness of returns in reflecting economic earnings is uncorrelated with the properties of accounting earnings. However, we focus *specifically* on the relation between the returns' timeliness in reflecting economic earnings and the biases in the accounting system. Proposition 3 states that firms in less conservative financial-reporting regimes are more likely to engage in timely preemptive voluntary disclosure than firms in more conservative regimes. This analysis suggests that, for the firms in relatively less conservative regimes, "returns lead earnings" as a result of their voluntary disclosures. For the relatively more conservative regimes, "earnings drive returns." Consequently, returns reflect the news of firms with less conservative accounting on a more timely basis than for firms with more conservative accounting.

Even though all firms in a given accounting jurisdiction are subject to a specific set of accounting standards, such standards may have different effects on the bias in different firms' financial reports. That is, the accounting standards in a given jurisdiction may lead to conservative reports for some firms but liberal reports for others. For example, U.S. GAAP is more conservative for firms heavily involved in R&D than for otherwise similar firms that are not. In contrast, U.S. GAAP is more liberal for firms that compensate their employees with employee stock options than for otherwise similar firms that do not. Our model can capture the resulting cross-sectional variation in the degree of conservatism in financial statements by allowing λ^H and λ^L to take on different values for different firms. Proposition 3 then predicts that firms that engage in timely voluntary disclosure will have less conservative financial-reporting systems than firms that do not. Let $\bar{\lambda}^H_D$ ($\bar{\lambda}^L_D$) and $\bar{\lambda}^H_N$ ($\bar{\lambda}^L_N$) be the average values of λ^H (λ^L) for the disclosing and the non-disclosing firms, respectively. By Proposition 3, $\bar{\lambda}^H_D \geq \bar{\lambda}^H_N$ and $\bar{\lambda}^L_D \leq \bar{\lambda}^L_N$.

To show how our model relates to the growing body of empirical evidence on the relation between accounting conservatism and the strength of the earnings-return relation, we focus on how the relation between conservatism and voluntary disclosure affects the following regression:

$$y = \alpha + \beta \Delta P + \epsilon, \tag{3}$$

where the dependent variable, y, is accounting earnings and the independent variable, ΔP, is the stock price change (or return) measured over the same period as y. Let β^+ and β^- represent the predicted values of β in equation (3) for non-negative and non-positive returns, respectively. This will allow us to examine how the relation between conservatism and disclosure incentives can lead to differences between the regression coefficients for positive vs. negative price changes.

Central to Basu's (1997) hypothesis are the assumptions that the timeliness of returns in reflecting economic events is uncorrelated with the resulting accounting earnings, and that λ^L is greater than λ^H. One way to ensure that regression (3) satisfies the first of these assumptions is to make certain that the measured return captures *all* the economic events reflected in the associated earnings. This would be difficult since accounting earnings in any period can reflect economic events that occurred many years before. However, if the returns measure does not capture all preemptive disclosures reflected in the associated earnings, then the correlation between the accounting bias and the firm's voluntary disclosure policy identified in our theory will generate the same predicted difference in the slope coefficients, *whether* λ^L *is greater than* λ^H. Thus, our theory suggests that, to accurately measure the degree of conservatism in an accounting regime by comparing β^- and β^+, one must control for the amount of voluntary disclosure across firms.

To demonstrate this we remove any potential scale difference between good and bad news from our model by assuming that the distribution of economic events (i.e., returns) is symmetric and that earnings scale good and bad news equivalently. Specifically, we assume that $x^H = -x^L \equiv x$, that $P^h = \frac{1}{2}$, and that $y^H = -y^L = x$. This results in a prediction of $\beta^+ = \beta^-$ when the timeliness of returns in capturing news is uncorrelated with the bias in the accounting system. However, our model predicts that returns are more timely in incorporating the news of firms with relatively less conservative accounting systems. As a result, our theory also predicts that $\beta^+ < \beta^-$, provided that the returns window does not capture *all* the economic events reflected in the associated earnings. Proposition 4 provides the predicted slope coefficient in an earnings-price change regression with earnings as the dependent variable when our results are applied to a sample of firms that differ in terms of the bias in their financial reports.[5]

Proposition 4: In a random sample of firms, in which some firms make preemptive disclosures before the returns window and others do not, the regression coefficient in Equation (3) is strictly greater for non-positive price changes than for non-negative price changes. That is, $\beta^- > \beta^+$.

The prediction in Proposition 4 is consistent with the empirical evidence in Basu (1997) and the related subsequent studies discussed above. However, the difference in slope coefficients that our model predicts is not attributable to a difference in the way the accounting system reports good vs. bad news for firms that do not make preemptive disclosures. Rather, our prediction stems from the manner in which biases in mandatory financial-reporting systems determine which firms optimally choose to make preemptive voluntary disclosures and which do not.

The intuition of Proposition 4 is as follows. Firms that issue preemptive disclosures outside the returns window used in the regression will experience smaller price change when they issue their financial statements than firms that do not make such preemptive disclosures. Accordingly, the returns of firms that make preemptive disclosures will tend to cluster in the middle of the returns distribution, while the firms that do not make preemptive disclosures will experience more extreme returns, both positive and negative. By Proposition 3, firms that make preemptive voluntary disclosures have relatively more liberal accounting systems than their non-disclosing counterparts, so (on average) their accounting earnings will be higher. The concavity of the earnings-returns relation Proposition 4 implies follows because the earnings are higher for the firms in the middle of the returns distribution than for the expected earnings of firms in the tails of the returns distribution.[6]

While our predictions are consistent with the empirical evidence reported by Basu (1997), Proposition 4 holds even when all firms' accounting systems exhibit a liberal bias—that is when $\bar{\lambda}_N^H$ and $\bar{\lambda}_D^H$ are greater than $\bar{\lambda}_N^L$ and $\bar{\lambda}_D^L$, respectively—as long as some firms engage in voluntary disclosures while others do not. Since the pattern occurs even when all firms have a liberal accounting bias, to the extent that the returns window does not capture all preemptive disclosures, one cannot unambiguously interpret evidence that β^- is smaller than β^+ in regression (3) as conservative financial reporting. The same precaution applies to interpretations of accounting systems ranked on comparisons of β^- and β^+.

The obvious way to control for the problem that the difference in β^- and β^+ may be attributable to differences in preemptive disclosure is to ensure that the returns window captures all preemptive disclosures. However, as the return window lengthens, the timing bias in a conservative accounting system disappears. So the strength of the hypothesized effect vanishes as the confidence in properly controlling for the alternative increases.

Another possible control would be to make sure the returns window contains only preemptive disclosures, and thus, that it excludes the dates of both the current and the previous earnings releases. For such an event window, our model predicts that it would be the returns of firms that *did not* make preemptive disclosures that tend to cluster in the middle of the returns distribution. Firms that do disclose, in contrast, should experience more extreme returns in this event window. Since firms that do not make voluntary disclosures have relatively more conservative accounting systems, their accounting earnings will be *lower* than the average for firms in the tails of the returns distribution. As long as there is no scaling difference in the way the accounting system reports good vs. bad news for disclosing firms, the inequality in Proposition 4 reverses, resulting in a convex relationship. Since Basu's (1997) hypothesis is unaffected by this change in the returns window, this alternative specification can be used to disentangle the two alternative explanations.

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[5] In deriving Proposition 4 we assume that all voluntary disclosures occur before the start of the returns window. Proposition 4 holds, however, as long as any of the voluntary disclosures take place prior to that time.

[6] This holds even if x does not become public for the non-disclosing firms when the firms release their financial reports. The only difference is that the price reaction for the non-disclosing firms upon the release of the financial report would be based on $E[x|y]$ rather than on x directly.

Basu (1997, Figure 1, Panel C) does include a test using a fiscal year returns window to exclude the market reaction to the current year's earnings release. Changing the returns window this way does not alter his prediction on the slope coefficients, and Basu (1997) finds that the relation between the slope coefficients in his initial regression is robust to this specification but is somewhat weaker. This suggests that his initial inferences could not be entirely explained by the omitted disclosure variable, and consequently the assumption that λ^L is greater than λ^H, on which he bases his hypothesis, appears valid.[7] Still, the apparent weakening of Basu's (1997) results when he uses a fiscal year returns window is consistent with preemptive voluntary disclosures explaining part of the difference in the slope coefficients, as our theory suggests.

V. CONCLUSION

In this paper we analyze the relation between the bias in a financial-reporting system and the incentives that owners optimally give managers to either preempt the financial reports by volunteering private information, or to withhold such information until the financial statement is made public. We develop a principal-agent model in which a potentially noisy and biased financial report becomes public at the end of the contracting horizon. By expending resources, the manager may become informed about the firm's economic conditions before the financial report is produced. The shareholders must then decide whether to rely exclusively on the financial report or to allow the manager to produce and disclose information at an earlier time.

In making this decision, shareholders trade off the cost of producing the information earlier against its benefits. Although such benefits may be numerous (including, for example, lowering the firm's cost of capital), we focus only on those related to evaluating managerial performance and thus improving managerial incentives. We show that incurring costs of early voluntary disclosure is optimal only when the firm's accounting system is not too conservative. Our results should help researchers incorporate the indirect informational effects into studies on the economic effects of changing mandatory disclosure requirements.

The key implication of our results is that, although greater conservatism in a financial-reporting regime may attenuate the observed relation between positive returns and earnings more so than that between negative returns and earnings, to the extent that the returns window does not include all preemptive disclosures, this relation may exist absent any conservatism in the financial-reporting regime. However, our theory does show how one may use an alternative regression specification to control for the omitted voluntary disclosure effect that we study.

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[7] Since the fiscal year returns window includes the market reaction to the prior year's earnings release, however, it is longer than the returns window we suggest here as a control for the effect of voluntary disclosures.

APPENDIX

Proof of Lemma 1

By standard arguments, both the IR and IC constraints are binding. The optimal solution characterized in Lemma 1 therefore can be obtained simply by solving these two equations for the two unknown contract points. Q.E.D.

Proof of Lemma 2

The proof consists primarily of five claims.

Claim (*i*): If TT^H is slack, then $U(s(\hat{x}^L, y^L)) = U(s(\hat{x}^L, y^H))$.

Proof: Suppose TT^H is slack and $U(s(\hat{x}^L, y^L)) < U(s(\hat{x}^L, y^H))$. Then lower $U(s(\hat{x}^L, y^H))$ by δ and increase $U(s(\hat{x}^L, y^L))$ by $\delta((1 - \lambda^L)/\lambda^L)$. This variation is neutral to the IR, IC, and TT^L constraints, and for sufficiently small δ does not violate the (slack) TT^H constraint. Therefore, this variation is feasible. Furthermore, because the manager is risk averse, this variation reduces the expected wage, contradicting the hypothesis that the optimal contract has TT^H slack and $U(s(\hat{x}^L, y^L)) < U(s(\hat{x}^L, y^H))$.

Suppose TT^H is slack and $U(s(\hat{x}^L, y^L)) > U(s(\hat{x}^L, y^H))$. Then lower $U(s(\hat{x}^L, y^L))$ by δ and increase $U(s(\hat{x}^L, y^H))$ by $\delta(\lambda^L/(1 - \lambda^L))$. Again, this variation is neutral to the IR, IC, and TT^L constraints and does not violate (slack) TT^H for sufficiently small δ. This contradicts the hypothesis that the optimal contract has TT^H slack and $U(s(\hat{x}^L, y^L)) > U(s(\hat{x}^L, y^H))$, completing the proof of the claim.

Claim (*ii*): If TT^L is slack, then $U(s(\hat{x}^H, y^L)) = U(s(\hat{x}^H, y^H))$.

Proof: Suppose TT^L is slack and $U(s(\hat{x}^H, y^L)) > U(s(\hat{x}^H, y^H))$. Then lower $U(s(\hat{x}^H, y^L))$ by δ and increase $U(s(\hat{x}^H, y^H))$ by $\delta((1 - \lambda^H)/\lambda^H)$. This variation is neutral to the IR, IC, and TT^H constraints and, for sufficiently small δ, does not violate the (slack) TT^L constraint. Therefore, this variation is feasible. Furthermore, because the manager is risk averse, this variation reduces the expected wage, contradicting the hypothesis that the optimal contract has TT^L slack and $U(s(\hat{x}^H, y^L)) > U(s(\hat{x}^H, y^H))$.

Suppose TT^L is slack and $U(s(\hat{x}^H, y^L)) < U(s(\hat{x}^H, y^H))$. Then lower $U(s(\hat{x}^H, y^H))$ by δ and increase $U(s(\hat{x}^H, y^L))$ by $\delta(\lambda^H/(1 - \lambda^H))$. Again, this variation is neutral to the IR, IC, and TT^H constraints and does not violate (slack) TT^L for sufficiently small δ. This contradicts the hypothesis that the optimal contract has TT^L slack and $U(s(\hat{x}^H, y^L)) < U(s(\hat{x}^H, y^H))$, completing the proof of the claim.

Claim (*iii*): The solution cannot have both constraints TT^H and TT^L slack.

Proof: If both TT^H and TT^L are slack, then by Claims (i) and (ii) these constraints can be written as:

$$U^H - U^L > -\frac{1 - \lambda^H}{\lambda^H}(U^H - U^L) \qquad TT^H$$

$$U^H - U^L < -\frac{\lambda^L}{1 - \lambda^L}(U^H - U^L) \qquad TT^L$$

where, by Claims (i) and (ii) respectively:

$$U^L \equiv U(s(\hat{x}^L, y^H)) = U(s(\hat{x}^L, y^L))$$
$$U^H \equiv U(s(\hat{x}^H, y^H)) = U(s(\hat{x}^H, y^L)).$$

Now, however, $U^H \geq U^L$ violates TT^L and $U^H \leq U^L$ violates TT^H. Therefore, a solution cannot have both constraints TT^H and TT^L slack.

Claim (*iv*): The solution to program TC has constraint TT^H slack and TT^L binding.

Proof: Suppose TT^H binds. Then Claim (iii) implies TT^L is slack. Furthermore, Claim (ii) implies $U^H \equiv U(s(\hat{x}^H, y^H)) = U(s(\hat{x}^H, y^L))$, so IC and TT^L can be written as:

$$U^H \geq \frac{v}{P^h - P^l} + \lambda^L U(s(\hat{x}^L, y^L)) + (1 - \lambda^L)U(s(\hat{x}^L, y^H)) \qquad \text{IC}$$

$$U^H < \lambda^L U(s(\hat{x}^L, y^L)) + (1 - \lambda^L)U(s(\hat{x}^L, y^H)) \qquad TT^L$$

generating a contradiction and proving TT^H slack. TT^L binding follows from Claim (iii).

Claim (*v*): The IR and IC constraints are both binding.

Proof: IR binds by the standard argument. If IC is slack, then $U(s(\hat{x}^H, y^H))$ can be lowered by δ and $U(s(\hat{x}^L, y^H))$ can be increased by $\delta(P^h\lambda^H/(1 - P^h)(1 - \lambda^L))$ to generate an improvement. Note that this variation is IR neutral, puts slack into TT^L, and by Claim (iv) satisfied TT^H provided that δ is sufficiently small.

To complete the proof of Lemma 2, solve the three binding constraints, IR, IC, and TT^L, for the three unknowns: $U^L \equiv U(s(\hat{x}^L, y^H)) = U(s(\hat{x}^L, y^L))$, $U(s(\hat{x}^H, y^H))$, and $U(s(\hat{x}^H, y^L))$. Q.E.D.

Proof of Proposition 1

Using Lemmas 1 and 2, we have:

$$\begin{aligned} E[G(U(s(y)))] = {} & P^h[\lambda^H G(U(s(y^H))) + (1 - \lambda^H)G(U(s(y^L)))] \\ & + (1 - P^h)[(1 - \lambda^L)G(U(s(y^H))) + \lambda^L G(U(s(y^L)))] \end{aligned}$$

and:

$$\begin{aligned} E[G(U(s(\hat{x}, y)))] = {} & P^h[\lambda^H G(U(s(y^H))) + (1 - \lambda^H)G(U(s(y^L)))] \\ & + (1 - P^h)G((1 - \lambda^L)U(s(y^H)) + \lambda^L U(s(y^L))). \end{aligned}$$

The difference is the contractual value of communication. Q.E.D.

Proof of Proposition 2

From Lemma 1 we have:

$$U(s(y^L)) =$$
$$\begin{cases} \underline{U} + v - \dfrac{vP^h}{P^h - P^l} - \dfrac{v(1 - \lambda^L)}{(P^h - P^l)(\lambda^H + \lambda^L - 1)} < \underline{U} + v - \dfrac{vP^h}{P^h - P^l} & \text{for } \lambda^L < 1, \\ \underline{U} + v - \dfrac{vP^h}{P^h - P^l} & \text{for } \lambda^L = 1. \end{cases}$$

Then, since by Lemma 1 $E[U(s(y))|x^L] = \underline{U} + v - vP^h/(P^h - P^l)$ and due to the convexity of $G(\cdot)$:

$$E[G(U(s(y)))|x^L] \begin{cases} > G\left(\underline{U} + v - \dfrac{vP^h}{P^h - P^l}\right) = G(U(s(\hat{x}^L, y))) & \text{for } \lambda^L < 1, \\ = G\left(\underline{U} + v - \dfrac{vP^h}{P^h - P^l}\right) = G(U(s(\hat{x}^L, y))) & \text{for } \lambda^L = 1. \end{cases}$$

Proposition 2 now follows from Proposition 1. Q.E.D.

Proof of Lemma 3

From Lemma 1 we have:

$$EU = \pi U(s(y^H)) + (1 - \pi)U(s(y^L))$$

and:

$$EG = \pi G(U(s(y^H))) + (1 - \pi)G(U(s(y^L))),$$

where:

$$\pi \equiv P^h\lambda^H + (1 - P^h)(1 - \lambda^L).$$

Holding the risk-premium constant while meeting (IR) implies $dEU/d\lambda^L = dEG/d\lambda^L = 0$. Thus:

$$\frac{d\pi}{d\lambda^L}\Delta U + \pi\frac{dU(s(y^H))}{d\lambda^L} + (1 - \pi)\frac{dU(s(y^L))}{d\lambda^L} = 0$$

and:

$$\frac{d\pi}{d\lambda^L}\Delta G + \pi\frac{dU(s(y^H))}{d\lambda^L}G'(U(s(y^H))) + (1 - \pi)\frac{dU(s(y^L))}{d\lambda^L}G'(U(s(y^L))) = 0,$$

where:

$$\Delta U \equiv [U(s(y^H)) - U(s(y^L))] \text{ and } \Delta G \equiv [G(U(s(y^H))) - G(U(s(y^L)))].$$

Multiplying $dEU/d\lambda^L$ by ΔG and $dEG/d\lambda^L$ by ΔU, and subtracting both sides of $dEG/d\lambda^L$ from both sides of $dEU/d\lambda^L$, after rearranging terms we obtain:

$$\pi\frac{dU(s(y^H))}{d\lambda^L}\left[\frac{\Delta G}{\Delta U} - G'(U(s(y^H)))\right] + (1 - \pi)\frac{dU(s(y^L))}{d\lambda^L}\left[\frac{\Delta G}{\Delta U} - G'(U(s(y^L)))\right] = 0. \tag{A1}$$

By convexity of $G(\cdot)$:

$$\left[\frac{\Delta G}{\Delta U} - G'(U(s(y^H)))\right] < 0 < \left[\frac{\Delta G}{\Delta U} - G'(U(s(y^L)))\right].$$

Thus:

$$\text{sign}\left(\frac{dU(s(y^H))}{\partial \lambda^L}\right) = \text{sign}\left(\frac{dU(s(y^L))}{\partial \lambda^L}\right).$$

Using the expressions in Lemma 1, we have:

$$\frac{dU(s(y^L))}{d\lambda^L} = \frac{v}{P^h - P^l}\left[\frac{1}{(\lambda^H + \lambda^L - 1)} + \frac{\left(\frac{d\lambda^H}{d\lambda^L} + 1\right)(1 - \lambda^L)}{(\lambda^H + \lambda^L - 1)^2}\right]$$

$$= \frac{v}{P^h - P^l} \cdot \frac{\lambda^H + \frac{d\lambda^H}{d\lambda^L}(1 - \lambda^L)}{(\lambda^H + \lambda^L - 1)^2}$$

and:

$$\frac{dU(s(y^H))}{d\lambda^L} = \frac{v}{P^h - P^l}\left[\frac{1}{(\lambda^H + \lambda^L - 1)} - \frac{\left(\frac{d\lambda^H}{d\lambda^L} + 1\right)\lambda^L}{(\lambda^H + \lambda^L - 1)^2}\right] = \frac{v}{P^h - P^l} \cdot \frac{\lambda^H - 1 - \frac{d\lambda^H}{d\lambda^L}\lambda^L}{(\lambda^H + \lambda^L - 1)^2},$$

which are both continuously differentiable and monotone in $d\lambda^H/d\lambda^L$ with:

$$\frac{dU(s(y^L))}{d\lambda^L} > 0 (< 0) \quad \text{for} \quad \frac{d\lambda^H}{d\lambda^L} > (<) \frac{\lambda^H}{\lambda^L - 1}$$
$$\frac{dU(s(y^H))}{d\lambda^L} > 0\ (< 0) \quad \text{for} \quad \frac{d\lambda^H}{d\lambda^L} < (>) \frac{\lambda^H - 1}{\lambda^L}.$$

Thus:

$$\text{sign}\left(\frac{dU(s(y^H))}{d\lambda^L}\right) = \text{sign}\left(\frac{dU(s(y^L))}{d\lambda^L}\right) \text{ requires either:}$$

$$\frac{\lambda^H - 1}{\lambda^L} > \frac{d\lambda^H}{d\lambda^L} > \frac{\lambda^H}{\lambda^L - 1} \text{ or } \frac{\lambda^H - 1}{\lambda^L} < \frac{d\lambda^H}{d\lambda^L} < \frac{\lambda^H}{\lambda^L - 1}.$$

And whenever $1 >> \{\lambda^H, \lambda^L\} >> \frac{1}{2}$, the first relationship is satisfied. Q.E.D.

Proof of Proposition 3

First, since:

$$E[U|x^L] = (1 - \lambda^L)U(s(y^H)) + \lambda^L U(s(y^L))$$

and:

$$E[G|x^L] = (1 - \lambda^L)s(y^H) + \lambda^L s(y^L),$$

we can write:

$$E[U|x^L] = \omega EU + (1 - \omega)U(s(y^L))$$
$$E[G|x^L] = \omega EG + (1 - \omega)s(y^L),$$

where:

$$0 < \omega = \frac{1 - \lambda^L}{\pi} = \frac{1 - \lambda^L}{P^h\lambda^H + (1 - P^h)(1 - \lambda^L)} < 1.$$

Then:

$$\begin{aligned}\frac{dE[G|x^L]}{d\lambda^L} &= \frac{d\omega}{d\lambda^L}[EG - G(U(s(y^L)))] + (1 - \omega)G'(U(s(y^L)))\frac{dU(s(y^L))}{d\lambda^L} \\ &= \frac{d\omega}{d\lambda^L}\pi\Delta G + (1 - \omega)G'(U(s(y^L)))\frac{dU(s(y^L))}{d\lambda^L}\end{aligned}$$

and:

$$\begin{aligned}\frac{dE[U|x^L]}{d\lambda^L} &= \frac{d\omega}{d\lambda^L}[EU - U(s(y^L))] + (1 - \omega)\frac{dU(s(y^L))}{d\lambda^L} \\ &= \frac{d\omega}{d\lambda^L}\pi\Delta U + (1 - \omega)\frac{dU(s(y^L))}{d\lambda^L}.\end{aligned}$$

From Lemmas 1 and 2, $\frac{dE[U|x^L]}{d\lambda^L} = 0$.

Therefore:

$$\begin{aligned}\frac{dE[G|x^L]}{d\lambda^L} &= \frac{dE[G|x^L]}{d\lambda^L} - \frac{dE[U|x^L]}{d\lambda^L}G'(U(s(y^L))) \\ &= \frac{d\omega}{d\lambda^L}\pi[\Delta G - \Delta U G'(U(s(y^L)))].\end{aligned}$$

Dividing both sides of this equation by $\pi\Delta U$ gives:

$$\text{sign}\left(\frac{dE[G|x^L]}{d\lambda^L}\right) = \text{sign}\left(\frac{d\omega}{d\lambda^L}\left[\frac{\Delta G}{\Delta U} - G'(U(s(y^L)))\right]\right)$$

By Lemma 3, $\frac{d\omega}{d\lambda^L} < 0$. $\frac{dE[G|x^L]}{d\lambda^L} < 0$ then follows from:

$$\frac{\Delta G}{\Delta U} > G'(U(s(y^L))) \text{ by the convexity of } G(\cdot).$$ Q.E.D.

Proof of Proposition 4

Running the regression:

$$y = \alpha + \beta\Delta P + \epsilon,$$

separately for non-negative and non-positive price changes, we can calculate the slope coefficients as:

$$\beta^+ = \frac{E(y|\Delta P > 0) - E(y|\Delta P = 0)}{(\Delta P|\Delta P > 0) - 0}, \quad \text{and} \quad \beta^- = \frac{E(y|\Delta P = 0) - E(y|\Delta P < 0)}{0 - (\Delta P|\Delta P < 0)}.$$

Here we have:

$$\begin{aligned} E(y|\Delta P > 0) &= \bar{\lambda}_N^H x - (1 - \bar{\lambda}_N^H)x = (2\bar{\lambda}_N^H - 1)x, \\ E(y|\Delta P = 0) &= \frac{x}{2}(\bar{\lambda}_D^H - (1 - \bar{\lambda}_D^H) + (1 - \bar{\lambda}_D^L) - \bar{\lambda}_D^L) \\ &= (\bar{\lambda}_D^H - \bar{\lambda}_D^L)x, \end{aligned}$$

and:

$$E(y|\Delta P < 0) = (1 - \bar{\lambda}_N^L)x - \bar{\lambda}_N^L x = (1 - 2\bar{\lambda}_N^L)x.$$

Thus:

$$\beta^+ = \frac{(2\bar{\lambda}_N^H - 1)x - (\bar{\lambda}_D^H - \bar{\lambda}_D^L)x}{x - 0} = 2\bar{\lambda}_N^H - (\bar{\lambda}_D^H - \bar{\lambda}_D^L) - 1,$$

and:

$$\beta^- = \frac{(\bar{\lambda}_D^H - \bar{\lambda}_D^L)x - (1 - 2\bar{\lambda}_N^L)x}{0 + x} = 2\bar{\lambda}_N^L + (\bar{\lambda}_D^H - \bar{\lambda}_D^L) - 1.$$

Thus:

$$\beta^+ - \beta^- = 2(\bar{\lambda}_N^H - \bar{\lambda}_N^L) - 2(\bar{\lambda}_D^H - \bar{\lambda}_D^L) < 0$$

where the inequality follows from $\bar{\lambda}_N^H < \bar{\lambda}_D^H$ and $\bar{\lambda}_N^L > \bar{\lambda}_D^L$ by Proposition 3. Q.E.D.

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the FASB's proposed extension to the standard business-reporting model includes quality measures (FASB 1996). However, there is widespread concern that traditional quality measures, which are financial in nature, do not directly capture the effect of poor product quality on intangibles such as customer goodwill, i.e., the customer's propensity to purchase products in the future (e.g., Deming 1982; Horngren et al. 2000, 687–688). Critics of traditional quality cost systems propose supplementing financial measures with *nonfinancial* quality measures, arguing that nonfinancial measures provide a better indication of quality-related customer goodwill losses. Investigating the empirical validity of these claims is critical, since one of the primary roles of product quality is to attract and retain customers. However, the evidence to date has largely been limited to theoretical and experimental studies. Nandakumar et al. (1993) theoretically demonstrate the advantages of nonfinancial quality measures relative to financial quality measures. In an experimental setting, Luft and Shields (2001) find that individuals can assess the future financial impact of quality more accurately using nonfinancial quality measures than using financial quality measures.

This paper provides some of the first archival empirical evidence on the extent to which financial and nonfinancial quality measures capture the effects of product quality on customer goodwill. Prior empirical accounting studies have typically focused on the financial impact of *quality practices* such as just-in-time (Balakrishnan et al. 1996) or total quality management (Ittner and Larcker 1995). In contrast, we focus on the information contained in specific measures of quality. We take as our premise that poor product quality reduces customer goodwill, which in turn reduces future revenues as dissatisfied customers take their business elsewhere or pay less for the firm's products. Accordingly, we test the extent to which reported quality measures are informative about future revenues by examining the extent to which changes in reported quality measures predict future changes in revenues.

Our analysis is based on a proprietary dataset of 11 manufacturing plants of a *Fortune 500* firm, compiled over 23 quarters. We find that financial quality measures such as external failure costs, which the firm incurs when products fail at the customer site, as well as nonfinancial quality measures such as defect rates and on-time deliveries, contain significant information about future sales, even after controlling for past sales. Our results also highlight the differential information contained in financial vs. nonfinancial metrics. While defect rates and on-time deliveries are associated with sales in the next quarter, external failure costs are negatively associated with sales two and three quarters ahead. One explanation for this result is that external failure costs result from customer complaints, signifying that the customer has reached out to the firm for assistance (e.g., Keaveney 1995; Smith et al. 1999). Since it usually takes time for the firm to provide failure recovery service, as well as for the customer to evaluate the quality of this service and make future purchasing decisions, there is a lag before external failures affect sales. This financial measure of quality is thus a leading indicator of sales farther into the future than are the nonfinancial measures. This finding is especially interesting since it contrasts with the popular notion that financial measures are lagging indicators (Ittner and Larcker 1998a).

Collectively, financial and nonfinancial quality measures increase the ability of our model to predict future sales by about 50 percent, compared to past sales alone. In contrast, prior studies find that survey measures of customer perception, such as customer satisfaction, are at best weak and mixed predictors of future sales. Ittner and Larcker (1998a) find that changes in customer satisfaction measures have *no* incremental information about future sales changes after controlling for past sales changes (at the 10 percent two-tailed level). Using two measures of customer satisfaction, Banker et al. (2000) find that changes in only one customer satisfaction measure predicts future revenue changes, albeit weakly (neither study discloses the incremental explanatory power of the customer satisfaction measures).

While there are several possible reasons for these weak results, Lambert (1998) conjectures that the measurement error in constructing customer satisfaction indices is a contributing factor. A business-to-business setting such as ours can further exacerbate the measurement error. The purchasing authority in customer firms is split among several employees, each of whom may have a different opinion of the supplier. Consequently, constructing a customer satisfaction index requires locating and querying all these employees and weighting their responses in proportion to their buying authority. In such situations, our findings suggest that measures of product and service quality, which are relatively easily and accurately measured, may be advantageous.

The management literature claims that because traditionally reported quality measures do not include intangible quality-related costs, managerial decisions based on these measures are often suboptimal (MacDuffie 1997). Survey evidence also supports this claim (Ittner and Larcker 1998b, Table 5). Our findings suggest that reported quality measures are not lacking in content; in fact, they convey a significant amount of information about future sales. However, since these measures do not directly quantify the cost of lost sales, managers must take the additional step of computing the future sales implications of these measures using regressions or other statistical methods.

The remainder of the paper is organized as follows. Section II describes the relevant prior literature and motivates our hypotheses. Section III provides economic and statistical evidence supporting the suitability of our research site and its quality measures for our tests. Section IV motivates the specification of our tests. Section V describes the empirical results and their implications, and Section VI concludes the paper.

II. HYPOTHESIS DEVELOPMENT

Traditional quality measurement systems divide quality-related costs into four categories (Kaplan and Atkinson 1998, 562): (1) *prevention* expenditures, such as equipment maintenance and design engineering, to prevent production of defective products; (2) *appraisal* expenditures that maintain quality levels through formal inspection, testing, and quality audits; (3) *internal failure* costs, such as the costs of scrap and rework, that arise from internal detection of quality problems; and (4) *external failure* costs, such as warranty and replacement costs and employee travel to customer sites, that arise from postshipment quality problems. Prevention and appraisal expenditures, collectively called conformance quality costs, reflect the costs of achieving a certain level of product quality. Internal and external failure costs, collectively called nonconformance costs, reflect the costs of not meeting quality standards. Our empirical tests focus on nonconformance costs.

Quality cost systems typically exclude important revenue effects of quality, namely, the opportunity costs of lost sales due to poor quality as well as the future revenue benefits when higher quality products and services increase customer satisfaction. For example, external failure costs include costs of product failure response, but not the effectiveness of these expenditures in resolving customer complaints or the opportunity costs of any lost sales. According to the marketing literature, many customers find complaining costly, and instead switch suppliers upon receiving poor quality products.[2] Consequently, external failure costs, which are triggered only when customers complain, further misstate the costs of shipping poor quality products.

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[2] For example, surveys indicate that only 5 percent of customers choose to complain upon receiving defective products (Kotler 1994, 41; also see Harari 1992). For a more formal mathematical model of the costs to customers of complaining, see Fornell and Wernerfelt (1987, 1988). Hansen et al. (1997) provide a survey of the marketing literature on customer complaint behavior.

Such limitations can lead managers to make incorrect decisions, thus imposing costs on the firm (Nandakumar et al. 1993; MacDuffie 1997). Including financial estimates of quality-related intangible costs can potentially overcome this limitation. However, it is difficult for managers to estimate directly these intangible costs, and more importantly, it is difficult for the accounting division to verify that the managers are not gaming these accruals. Most financial quality measurement systems therefore omit these intangible costs and benefits. Many authors claim that nonfinancial quality measures serve as a useful alternative because they provide direct and *reliable* measures of these quality-related intangible costs (Nandakumar et al. 1993; Ittner and Larcker 1998b; Horngren et al. 2000, 687). For example, outgoing defect rates directly measure outgoing product quality, and unlike external failure costs, are independent of the customer's decision to complain. Another important aspect of quality is supplier service. Suppliers that deliver products on time and resolve problems quickly are more likely to receive future orders (Nandakumar et al. 1993; Kaplan and Atkinson 1998, 558–561). Supplier timeliness is best measured using nonfinancial measures such as on-time deliveries. Trade literature surveys indicate that defect rates and on-time deliveries are the most important criteria for supplier selection, eclipsing even price in significance (Fitzgerald 1998, 1999).

It is not clear, however, whether these perceived advantages of nonfinancial quality measures can overcome the shortcomings of financial quality measures. Nonfinancial measures may not accurately capture customers' subjective perceptions of the product that prompt them to make future purchases. For instance, defect rate measures may not identify the kinds of defects that matter to customers, and some late deliveries may be inconsequential. On the other hand, financial quality measures could capture activities strongly correlated with future lost sales. Consequently, the relative roles played by financial and nonfinancial quality measures in predicting future revenue information are open empirical questions.

We examine directly the relative ability of financial and nonfinancial quality metrics to capture the effects of product quality on future sales. Since quality problems do not affect sales revenues immediately, we test the extent to which financial quality measures are leading indicators of future sales. We also investigate the extent to which nonfinancial measures play an incremental role in explaining future sales. Specifically, we hypothesize:

H1: Reported financial quality measures are leading indicators of future sales.

H2: Reported nonfinancial quality measures have incremental power to explain future sales, even after controlling for financial quality measures.

III. RESEARCH SITE

Our research site comprises 11 plants belonging to a manufacturing group of a *Fortune 500* firm. The plants manufacture industrial and thermostatic clad metals, as well as a range of electromechanical and electronic control products, such as motor protectors, relays, circuit breakers, automotive engine controls, and thermostats. The two main technologies underlying these products are clad-metal technology, which bonds different metals to achieve the required electrical and thermostatic attributes, and semiconductor technology, which creates electronic control devices based on these metals. The products have applications in industries ranging from aerospace, appliance, and automotive to coinage and electronics.

There are several reasons why this research site is well suited to our analyses. First, similar products, common manufacturing technologies, and a common quality cost measurement system minimize the heterogeneity problems prior research encountered from pooling across multiple firms and industries (e.g., Ittner 1996). Second, technological maturity implies minor product and process changes over time, alleviating concerns about large structural time-series shifts in the data. Finally, evidence on the forward-looking nature of nonfinancial quality measures in this setting would indicate that nonfinancial measures are useful beyond the high-tech, high-growth industries in which financial measures are widely perceived to be uninformative (Amir and Lev 1996).

Given the importance of product quality in the electronics industry, our research site used a detailed quality measurement system for both planning and performance evaluation. The research site created this system carefully, soliciting responses from both divisional management and plant personnel, most of whom had undergone extensive quality training programs offered by outside experts. As shown in Table 1, this quality measurement system tracks common financial and nonfinancial quality measures. The financial measures include costs of quality-related engineering, marketing, product design, and preventive maintenance, as well as quality planning, inspection, scrap, rework, and warranty and return costs. The nonfinancial measures include outgoing defect rates and the number of on-time deliveries. The firm estimated outgoing defect rates, measured as the number of defective parts per million from an audit inspection of a statistical sample of completed products. The accounting department managed the quality measurement system with an extensive system of charge codes and audits to ensure reliability. Furthermore, to facilitate comparisons of current quality costs with those from prior years, the accounting department used the same measurement process over the time period of this study. These features suggest that the quality data are highly reliable and comparable.

The group provided data for 23 quarters, starting with the fifth year of the cost of quality program. The short purchase cycle of these products makes quarterly data more appropriate than annual data for examining the lead-lag relations between reported quality costs and sales. As interviews with management confirmed, starting with the fifth year also eliminates possible implementation problems in the measurement system.

Table 1 provides descriptive statistics on all quality measures.[3] For both the first and last quarters of our sample, internal failure costs represent the highest dollar value category of quality costs, followed by prevention, appraisal, and external failure. For the first quarter of our sample, total quality costs comprise 7.98 percent of sales. For the one plant for which we had the data, operating margins averaged approximately 40 percent. Assuming similar operating margins for all the plants suggests that reported quality costs approximate an economically significant 20 percent of operating margins.

We next examine how the quality measures change over time. Table 2 presents the results. After controlling for across-plant variation using plant-level dummies, prevention, appraisal, and internal failure costs (computed as percentage of sales and controlled for inflation) all decrease with time (t-statistics = −3.90, −7.16, and −1.93, respectively). Additionally, defect rates decrease with time (t-statistic = −10.01). The decreases in quality

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[3] We follow the research site's categorization of prevention, appraisal, internal failure, and external failure costs as shown in Table 1. Given the comprehensive measurement system and its extensive usage in the firm, it is unlikely that the research site's classification miscategorizes the costs. Since each plant produces multiple related product lines, the quality measures are aggregated over all the product lines for each plant. Because the plants manufacture very similar products, aggregation is unlikely to pose a problem. Finally, we adjust all the cost data for inflation using the producer price index.

TABLE 1
Distribution of Quality Measures for 11 Plants of a *Fortune 500* Manufacturing Firm over 23 Quarters[a]

Quality Cost Category (% of contemporaneous sales)	*Mean (Median) in the First Quarter*	*Mean (Median) in the Last Quarter*	*Standard Deviation over All Quarters*
Prevention			
Quality engineering	0.70	0.91	0.57
Receiving inspection	0.24	0.22	0.21
Preventive repair/maintenance	0.43	0.31	0.34
Process engineering	0.32	0.38	0.26
Design engineering	0.38	0.27	0.38
Quality training	0.13	0.14	0.23
Total Prevention	2.20 (2.12)	2.23 (2.17)	0.66
Appraisal			
Lab audit[b]	0.42	0.40	0.37
Design analysis	0.24	0.17	0.23
Product acceptance	0.77	0.63	0.53
Manufacturing inspection	0.41	0.32	0.31
Total Appraisal	1.84 (1.70)	1.52 (1.39)	0.78
Internal Failure			
Scrap	2.84	2.43	1.25
Rework	0.58	0.42	0.49
Process engineering	0.15	0.18	0.17
Total Internal Failure	3.57 (3.20)	3.03 (2.79)	1.52
External Failure[c]			
Returned material	0.20	0.29	0.35
Marketing	0.05	0.05	0.04
Process engineering	0.07	0.08	0.11
Repair	0.02	0.01	0.02
Travel	0.03	0.03	0.05
Total External Failure	0.37 (0.31)	0.46 (0.39)	0.34
Total Failure Costs	7.98 (7.49)	7.24 (6.77)	1.94
Defect Rates[d]	3307 (2105)	1331.5 (909)	2181.5
Number of On-Time Shipments	5263 (4056)	5202.8 (5133.5)[e]	5062.1

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[a] We deflated costs and sales by the industry producer price index.

[b] In lab audits, workers test a sample of products under simulated use conditions in an off-line inspection laboratory.

[c] One component of external failure that we exclude (including this table as well) is liability claims. Liability claims are infrequent and have little relation to the quarter in which the quality problem occurred.

[d] Defect rates are the number of defective parts per million identified at final audit.

[e] This figure is calculated for the 21st quarter because later data are not available.

TABLE 2
Time-Series Regressions of Quarterly Quality Costs, Defect Rates, and On-Time Delivery for 11 Plants of a *Fortune 500* Manufacturing Firm over 23 Quarters

	Prevention$_t$ as % of Sales$_t$	*Appraisal$_t$ as % of Sales$_t$*	*Internal Failure$_t$ as % of Sales$_t$*	*External Failure$_t$ as % of Sales$_t$*	*Log of Defect Rates$_t$*	*On-Time Deliveries$_t$ as % of Sales$_t$*	*Log of Sales$_t$*
Time trend (t)	−0.016***	−0.029***	−0.017*	0.002	−0.044***	−0.0008***	0.013***
	(−3.90)	(−7.16)	(−1.93)	(0.53)	(−10.01)	(−2.65)	(6.72)
n	253	253	253	253	253	173	253
Adjusted R^2	0.57***	0.74***	0.70***	0.24***	0.82***	0.95***	0.92***

***, **, * Statistically significant at the 1 percent, 5 percent, and 10 percent levels (two-tailed), respectively.
t-statistics in parentheses.
The regression includes plant dummies, which are not reported.

costs and improvement in defect rates suggest that the firm has learned to produce higher quality products at lower costs (Li and Rajagopalan 1998).[4] Interviews confirmed that managers perceive that customer demand increases with product quality; as a result, the firm continuously experiments with various ideas to improve product quality and increase future sales. Thus, the time-series variations in the quality data are likely to affect future sales, resulting in powerful tests of our hypotheses.

The coefficients in Table 2 indicate that the prevention and appraisal costs (as a percentage of sales) decrease by approximately 0.02 percent and 0.03 percent, respectively, each quarter. Since prevention and appraisal costs are approximately 2 percent of sales on average (Table 1), their decrease (unscaled) is about 1 percent each quarter. Converting the coefficients on the logs of defect rates and sales into percentage changes, the time-series drift in defect rates and sales is also small (5 percent and 1 percent, respectively), suggesting that the firm is on a continuous quality improvement path.

Finally, industry shocks can change the firm's pricing policies and affect our dependent variable, sales. Since we have no data on the firm's specific pricing policies, we examine the industry's structure and financial performance using Compustat data. Pricing and volume shocks are typically associated with changes in the costs, profitability, and competitive structure of the industry. Observation of minimal changes reduces the need for explicit controls for demand shocks. Thus, we examined the corresponding five years of financial ratios for the 3-digit SIC code industry to which our firm belonged. We found no apparent changes in industry averages of costs of goods sold and sales (as a percentage of total firm assets) or industry ROA. The number of firms in the industry also did not change over the five years, and the yearly Herfindahl index, which measures industry concentration, ranged from 0.12 to 0.13. These facts suggest a stable and mature industry, thus mitigating the need to control for explicit pricing and other industry-level shocks in the sales regression.

IV. MODEL SPECIFICATION

Our regressions have three key specification issues: (1) the choice of the regressors, (2) the form of the regressors (levels vs. changes), and (3) the lag length. With respect to the regressors, external failure costs are a measure of customer experience with the product. Internal failure costs also reflect outgoing product quality.[5] In contrast, prevention and appraisal costs represent expenditures for improving product quality, but it is the effectiveness of these expenditures, not the amounts themselves, that determines product quality. Consequently, our main tests include only failure costs as the financial regressors (we later include prevention and appraisal cost measures as a sensitivity check). Both defect rates and on-time deliveries are direct nonfinancial measures of product and service quality; thus, we include both of these measures.[6]

There are several reasons why we conduct our regressions in changes rather than levels. First, variations in levels in panel data can proxy for plant effects or other correlated variables. Second, Table 2 documents a downward drift in quality costs and defect rates, suggesting that levels could also proxy for a time trend. The plant-level and time trend effects vanish in the changes or first-difference specifications. Finally, levels are typically highly correlated with their lags, precluding tests that use multiple lags.

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[4] However, it appears that the firm has been unable to improve external failure costs or on-time deliveries over time. The on-time delivery regression in Table 2 has fewer observations than the remaining regressions because on-time delivery data are not available for all quarters. Section V discusses this issue in greater detail.

[5] Internal failure costs can be positively or negatively associated with outgoing product quality. Positive association can result if high internal failures indicate some fundamental quality problem that has not been fully solved. Negative association can result if management identifies and repairs all defective units internally.

[6] Defect rates are typically subject to measurement, testing, and sampling errors. Moreover, to the extent that the final audit indicates an unacceptably high defect rate, the firm might suspend outgoing shipments, causing reported defect rates to deviate from the true outgoing product quality. Consequently, both internal failures and outgoing defect rates can be informative about the true quality of outgoing products as experienced by customers.

Since quality costs increase with volume, we control for past changes in sales to abstract from the effect of changes in volume on quality cost measures. Moreover, past sales control for other unidentified determinants of future sales changes that past quality measures do not capture, such as seasonal effects.

Our sample plants' annual sales range from \$3.6 million to \$95.8 million (mean = \$35.9 million). Larger plants are likely to have larger quarterly changes in costs, sales, and number of on-time shipments. To control for this heteroskedasticity, we scale the variables by plant sales. Although scaling by production volume would be ideal, we do not have this data. However, the research site had a well-coordinated sale and production process and large inventory build-ups were rare, suggesting that aggregate sales are a reasonable proxy for production volume.[7] We deflate the changes in quality costs, the number of on-time deliveries, and sales using prior period sales to compute the following measures:

$$\Delta\text{Quality Cost}_t = \frac{\text{Quality Cost}_t - \text{Quality Cost}_{t-1}}{\text{Sales}_{t-1}},$$

$$\Delta\text{On-time}_t = \frac{\text{Number of on-time deliveries}_t - \text{Number of on-time deliveries}_{t-1}}{\text{Sales}_{t-1}}, \text{ and}$$

$$\Delta\text{Sales}_t = \frac{\text{Sales}_t - \text{Sales}_{t-1}}{\text{Sales}_{t-1}}.$$

Quality costs and on-time deliveries are absolute measures. However, our research site measures defect rates in parts per million. Given this implicit scaling by production volume, we do not deflate the defect rates by plant-size metrics, and compute changes in defect rates as:

$$\Delta\text{Defect Rate}_t = \text{Defect Rate}_t - \text{Defect Rate}_{t-1}.$$

Our empirical tests regress ΔSales_t on $\Delta\text{Quality Measure}_{t-k}$ and ΔSales_{t-k} $(4 \geq k \geq 1)$. Our scaling technique assures that the associations between current sales and past quality measures are not mechanical scaling artifacts. The deflator of the dependent variable, ΔSales_t, is Sales_{t-1}. However, we scale the quality regressors, $\Delta\text{Quality Measure}_{t-k}$, by Sales_{t-k-1}. Thus, neither Sales_{t-1}, the deflator for the dependent variable, nor Sales_t, a component of the numerator of the dependent variable, is present in the quality regressors.

The relatively short purchase cycle of the firm's commodity products suggests that customer dissatisfaction will quickly affect future sales. Thus, we use both one-quarter and four-quarter lags of quality regressors.

V. RESULTS

Table 3 provides a full correlation matrix of the regressors. The correlation between any two variables X_{t-1} and Y_{t-2} (or X_{t-2}) is similar to the correlation between X_{t-2} and

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[7] Group managers noted an additional advantage of using sales as the volume indicator for quality cost measurement. Unlike production volumes, which weight all the products equally, sales volumes weight products by level of manufacturing complexity (which is reflected in manufacturing costs and sales prices), producing an equivalent-unit volume measure. Furthermore, sensitivity tests reported later reveal that our results are not an artifact of our scaling procedure.

TABLE 3

Correlations among Quarterly Changes in Quality Costs, Defect Rates, Number of On-Time Shipments, and Sales for 11 Plants of a *Fortune 500* Manufacturing Firm over 18 Quarters

	ΔEF_{t-1}	ΔEF_{t-2}	ΔEF_{t-3}	ΔEF_{t-4}	ΔIF_{t-1}	ΔIF_{t-2}	ΔIF_{t-3}	ΔIF_{t-4}	ΔD_{t-1}	ΔD_{t-2}	ΔD_{t-3}	ΔD_{t-4}	ΔO_{t-1}	ΔO_{t-2}	ΔO_{t-3}	ΔO_{t-4}	ΔS_{t-1}	ΔS_{t-2}	ΔS_{t-3}
ΔEF_{t-2}	−0.43																		
ΔEF_{t-3}	0.03	−0.46																	
ΔEF_{t-4}	−0.16	0.02	−0.25																
ΔIF_{t-1}	−0.03	0.18	−0.16	−0.06															
ΔIF_{t-2}	0.01	−0.03	0.14	0.06	0.25														
ΔIF_{t-3}	0.04	0.01	0.02	0.06	−0.21	−0.24													
ΔIF_{t-4}	−0.07	0.05	−0.01	0.12	−0.03	−0.22	−0.25												
ΔD_{t-1}	0.02	0.15	−0.10	−0.09	−0.07	0.05	0.06	0.15											
ΔD_{t-2}	−0.10	0.02	0.10	0.04	0.07	−0.10	0.08	0.05	−0.30										
ΔD_{t-3}	0.10	−0.10	0.01	0.24	−0.10	0.06	−0.09	0.08	0.07	−0.31									
ΔD_{t-4}	0.12	0.10	−0.05	−0.21	0.21	−0.08	0.05	−0.08	−0.08	0.08	−0.33								
ΔO_{t-1}	−0.43	0.01	0.01	0.00	0.29	0.08	−0.02	0.01	0.14	−0.06	0.03	0.05							
ΔO_{t-2}	0.34	−0.43	0.01	0.01	−0.12	0.29	0.08	−0.02	0.09	0.14	−0.06	0.03	−0.21						
ΔO_{t-3}	−0.10	0.34	−0.43	0.01	−0.03	−0.12	0.29	0.08	0.01	0.09	0.14	0.06	0.02	−0.21					
ΔO_{t-4}	0.07	−0.11	0.18	−0.05	−0.07	−0.13	−0.06	0.32	0.21	−0.09	0.07	0.25	0.14	0.02	−0.21				
ΔS_{t-1}	0.45	−0.25	0.00	0.03	0.37	0.04	−0.16	−0.08	−0.11	0.02	0.06	0.04	0.09	0.19	−0.19	0.06			
ΔS_{t-2}	−0.12	0.45	−0.27	−0.05	0.04	0.37	0.04	−0.15	0.09	−0.11	0.01	0.04	0.03	0.09	0.19	−0.23	−0.23		
ΔS_{t-3}	0.01	−0.12	0.41	−0.12	−0.27	0.01	0.41	0.00	0.05	0.06	−0.11	0.06	−0.02	0.03	0.09	0.07	−0.17	−0.23	
ΔS_{t-4}	−0.09	0.00	0.04	0.20	−0.01	−0.26	−0.02	0.50	0.13	0.11	0.08	−0.19	0.02	−0.02	0.03	0.36	−0.12	−0.22	−0.12

Correlations with magnitudes greater than 0.14 are significant at the 10 percent level (two-tailed).

We scale quarterly changes in external and internal failure costs (EF and IF, respectively), the number of on-time shipments (O), and sales (S) by prior period plant sales. Defect rate changes (ΔD) are changes in defect rates measured in parts per million.

Y_{t-3} (or X_{t-3}), suggesting temporal stability in the data. Furthermore, since the correlation magnitudes never exceed 0.50, multicollinearity is unlikely to affect our inferences (variance inflation factors [VIF] confirm this assertion). Finally, quality costs correlate positively with contemporaneous sales changes, justifying the inclusion of past sales changes as regressors to control for any volume implications.

Results with One-Quarter Lag

Table 4 provides the results of regressing future sales on past sales and past quality measures. Table 4 uses a one-quarter lag for the quality measures, but lags past sales changes by both one and four quarters to control for seasonal effects. The first column shows that the adjusted R^2 using past sales only is 25 percent, significant at the 1 percent level. Both one-quarter and four-quarter sales lags are significant, indicating mean-reverting seasonal effects.[8]

We next add failure costs to the regression. The R^2 increases by 0.07, from 25 percent to 32 percent. As expected, external failures are significantly negatively associated with future sales. The third column in Table 4 adds nonfinancial quality measures, which further increase the R^2 by 0.06, to 38 percent. Improvements in defect rates and on-time deliveries are positively associated with future sales, suggesting that customers value both quality and timeliness.

These results suggest that financial quality measures contain significant forward-looking sales information beyond current and past sales, and nonfinancial measures have significant additional forward-looking sales information beyond financial quality measures. However, the inclusion of nonfinancial measures makes failure costs insignificant. Further analysis (untabulated) reveals that on-time deliveries drives this insignificance (as would be expected given the Table 3 evidence that defect rates, unlike on-time deliveries, are not correlated with contemporaneous failure costs). The importance of on-time deliveries is also reflected in trade surveys of buyer-supplier relations in the aerospace industry (a buyer industry for our firm), which indicate that on-time deliveries are among the most valued supplier attributes, even eclipsing price (Fitzgerald 1998, 1999). Most buyers follow just-in-time manufacturing practices, so shipment delays or mistakes are especially disruptive and costly.

We cannot conclude from Table 4 that nonfinancial quality measures fully subsume the future revenue information in financial quality measures. First, the regression uses a single-period lag, and thus cannot determine whether the association between quality measures and sales persists. For example, the associations could be temporary, and reverse in a subsequent period. Second, a single-period lag does not capture delayed associations, which are especially likely to arise in response to external failures. External failures give the firm an opportunity to remedy the complaint and win back the customer. Prior empirical evidence indicates that failure to respond meaningfully to this opportunity leads to customer defections (Keaveney 1995). However, since it usually takes time both for the firm to provide failure recovery service and for the customer to evaluate the quality of this service, there is likely be a lag before external failures affect sales.

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[8] Note that we have only 140 plant-quarter observations, fewer than the number of observations in Table 2. This decrease occurs because of first differencing and the use of lags. Additionally, on-time deliveries are not available for all plants in all periods. Except for three observations (which we imputed), all the missing data points for on-time deliveries occurred at the beginning or the end of the time series. The means of the independent variables across the observations used in the regression were not significantly different from the respective means from the full sample, suggesting that the truncation of the sample used in the regression is not systematic. Additionally, to ensure the comparability of the results across various regressions, we run all regressions on the same truncated dataset.

TABLE 4

OLS Regressions of Changes in Sales on One-Quarter Lagged Changes in Quality Measures for 11 Plants of a *Fortune 500* Manufacturing Firm over 18 Quarters

	Dependent Variable: $\Delta Sales_t$			
Regressors	*Regression (1)*	*Regression (2)*	*Regression (3)*	*Regression (4)*
Intercept	0.014 (1.23)	0.014 (1.30)	0.007 (0.69)	0.005 (0.52)
ΔExternal Failure$_{t-1}$		−5.302*** (−2.98)	−1.386 (−0.69)	
ΔInternal Failure$_{t-1}$		3.489** (2.37)	2.158 (1.49)	
ΔDefect Rate$_{t-1}$			−0.00003** (−2.37)	−0.00003*** (−2.89)
ΔOn-time$_{t-1}$			0.236*** (3.52)	0.283*** (5.18)
ΔSales$_{t-1}$	−0.211*** (−2.84)	−0.161* (−1.85)	−0.231*** (−2.71)	−0.224*** (−3.31)
ΔSales$_{t-4}$	0.477*** (6.28)	0.451*** (6.25)	0.482*** (6.91)	0.493*** (7.10)
n	140	140	140	140
Adjusted R^2	0.25***	0.32***	0.38***	0.38***

***, **, * Statistically significant at the 1 percent, 5 percent, and 10 percent levels (two-tailed), respectively.
t-statistics in parentheses.
We scale quarterly changes in internal and external failure costs, number of on-time shipments, and sales by prior period plant sales. Defect rate changes are changes in defect rates measured in parts per million.

Results with Four-Quarter Lags

To address these limitations of the parsimonious models in Table 4, Table 5 presents the results of regressions estimated with four lags of all the regressors (a full year is sufficient given the short cycle-time in this industry). The results are similar to those in Table 4 in many respects. Four lags of past sales changes still explain 25 percent of current sales. Adding four lags of past financial quality measures improves R^2 to 34 percent, similar to the one-lag case. Adding nonfinancial measures further increases the adjusted R^2 to 44 percent. Thus, financial measures by themselves contain information about future sales, and nonfinancial measures augment this information significantly.

Attesting to the stability of the results, the signs and the magnitudes of the one-lag coefficients in Table 5 that are significant at the 10 percent two-tailed level are consistent with the signs and the magnitudes of the significant coefficients in Table 4. Furthermore, sales still exhibit seasonal effects; single-lag defect rates and on-time deliveries are negatively and positively, respectively, associated with future sales; and, finally, in the absence of nonfinancial measures, single-lag external and internal failures are negatively and positively, respectively, associated with future sales.

If poor product quality causes only temporary losses in sales, we should observe positive relations between ΔSales_t and $\Delta \text{Defect Rate}_{t-2}$ (or $\Delta \text{Defect Rate}_{t-3}$), and negative relations between ΔSales_t and $\Delta \text{On-time}_{t-2}$ (or $\Delta \text{On-time}_{t-3}$) as sales rebound in later periods. However, the coefficients on *both* on-time deliveries and defect rates are significant for one lag, and mostly insignificant for two lags and more, thus implying that defects and late deliveries lead to changes in sales *levels* that persist for at least a year.[9] These results further attest to the strong effects of product quality on future revenues and to the ability of nonfinancial measures to capture these effects.

Defect rates and on-time deliveries rapidly affect sales. Most of the effect arises in a single lag.[10] In this commodity industry, customers quickly recognize product and delivery problems. They can either complain (an activity captured by our external failure cost regressor) or simply switch suppliers. Given the competitive nature of the industry, customer can switch suppliers relatively quickly. Thus, defect rates and on-time deliveries, which proxy for customer switches once external failure costs are controlled for, have significant and immediate sales effects.

In contrast to Table 4, when we allow more lags, on-time deliveries do not completely subsume external failure costs. Although the coefficient on external failures is not significant in period $t - 1$, external failure coefficients are significantly negative in periods $t - 2$ and $t - 3$ (Table 5, Column 3, coefficients = -13.7 and -12.3, and t-statistics = -1.82 and -2.20).[11]

The significantly negative delayed effect of external failures on sales suggests that the firm was unable to respond satisfactorily to customers who took the initiative to complain, and therefore eventually lost sales. This result is not surprising since the firm's quality improvement projects had an internal production-process orientation, rather than an external customer-service orientation.[12] The programs focused on producing high-quality products by improving the production process ("getting things right the first time"), rather than providing effective failure recovery service to the customer. The plants had relatively tight production schedules, and thus were not well prepared to handle customer returns efficiently. The firm had devoted little attention to improving paperwork, rebilling, and other administrative and support activities for handling customer complaints. Group managers suspected considerable delays and mistakes in these activities.[13] Consequently, service recovery typically took one or two quarters. In addition, customers need time to evaluate this service and switch suppliers if necessary. Thus, our finding of a two- to three-quarter delay between external failures and future sales is consistent with company practices.

External failures have substantial effects on future sales. As a first-order approximation, a \$1 increase in external failure costs is associated with a sales decrease of \$13.70 and

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[9] The four-quarter lag of defect rates is significantly negative, further strengthening the persistence argument.

[10] The four-quarter lag of defect rates is also significant, but the coefficient is about half the magnitude of the single-quarter lag, and the significance is much smaller.

[11] In contrast, Table 5 shows that except for the fourth lag, on-time deliveries completely subsume internal failures. We do not have an explanation for the significance of the fourth lag, other than a conjecture that it indicates some seasonal effect not captured by the sales regressors. More importantly, however, the coefficients on internal failures switch signs, suggesting that internal failure costs do not appear to have a lasting incremental effect on future sales.

[12] In fact, marketing surveys indicate that only 50 percent of the customers who complain report a satisfactory problem resolution (Kotler 1994, 479). Other surveys indicate that the manufacturing industry, in particular, places little emphasis on customer service (Executive Knowledgeworks 1989).

[13] Hart et al. (1990) argue that these support activities are critical, but often neglected, parts of effective failure recovery practices. The research site had recently begun efforts to measure the effect of these support activities through "Hidden Factory" reviews.

TABLE 5
OLS Regressions of Changes in Sales on Four-Quarter Lagged Changes in Quality Measures for 11 Plants of a *Fortune 500* Manufacturing Firm over 18 Quarters

	Dependent Variable: $\Delta Sales_t$			
Regressors	*Regression (1)*	*Regression (2)*	*Regression (3)*	*Regression (4)*
Intercept	0.025* (1.91)	0.014 (1.08)	0.003 (0.21)	0.010 (0.77)
ΔExternal Failure$_{t-1}$		−6.478*** (−3.27)	1.806 (0.65)	
ΔExternal Failure$_{t-2}$		−6.619* (−1.71)	−13.705* (−1.82)	
ΔExternal Failure$_{t-3}$		−7.8* (−1.82)	−12.301** (−2.20)	
ΔExternal Failure$_{t-4}$		−5.448 (−1.45)	−5.891 (−1.33)	
ΔInternal Failure$_{t-1}$		3.410** (2.05)	2.759 (1.53)	
ΔInternal Failure$_{t-2}$		1.165 (0.63)	0.669 (0.33)	
ΔInternal Failure$_{t-3}$		−1.344 (−0.74)	−2.088 (−1.05)	
ΔInternal Failure$_{t-4}$		−3.471** (−2.06)	−4.361** (−2.47)	
ΔDefect Rate$_{t-1}$			−0.00004*** (−3.11)	−0.00004*** (−3.33)
ΔDefect Rate$_{t-2}$			−0.000003 (−0.25)	−0.000005 (−0.36)
ΔDefect Rate$_{t-3}$			0.00001 (1.00)	0.000007 (0.58)
ΔDefect Rate$_{t-4}$			−0.00002* (−1.74)	−0.00001 (−0.93)
ΔOn-time$_{t-1}$			0.318*** (3.92)	0.305*** (5.22)
ΔOn-time$_{t-2}$			−0.037 (−0.47)	−0.021 (−0.28)
ΔOn-time$_{t-3}$			−0.078 (−1.08)	−0.092 (−1.27)
ΔOn-time$_{t-4}$			−0.036 (−0.52)	−0.082 (−1.15)

(*Continued on next page*)

TABLE 5 (Continued)

	Dependent Variable: $\Delta Sales_t$			
Regressors	*Regression (1)*	*Regression (2)*	*Regression (3)*	*Regression (4)*
$\Delta Sales_{t-1}$	−0.261*** (−3.25)	−0.160 (−1.65)	−0.276*** (−2.65)	−0.199** (−2.61)
$\Delta Sales_{t-2}$	−0.110 (−1.16)	−0.070 (−0.66)	0.003 (0.03)	−0.014 (−0.14)
$\Delta Sales_{t-3}$	−0.119 (−1.39)	0.065 (0.63)	0.058 (0.57)	−0.029 (−0.32)
$\Delta Sales_{t-4}$	0.424*** (5.00)	0.590*** (6.10)	0.688*** (6.88)	0.544*** (5.98)
n	140	140	140	140
Adjusted R^2	0.25***	0.34***	0.44***	0.39***
F-statistic for External Failures		3.01**	2.03*	
F-statistic for Internal Failures		2.82**	3.20**	
F-statistic for Defect Rates			3.67***	3.09**
F-statistic for On-Time Deliveries			4.73***	7.89***

***, **, * Statistically significant at the 1 percent, 5 percent, and 10 percent levels (two-tail), respectively.
t-statistics in parentheses.
We scale quarterly changes in prevention, appraisal, internal and external failure costs, number of on-time shipments, and sales by prior period plant sales. Defect rate changes are changes in defect rates measured in parts per million.

$12.30 in the second and third subsequent quarters, respectively.[14] Our data for one plant indicate a 40 percent operating margin, implying that a $1 increase in external failure costs translates to approximately $10.40 in lost profits.

Our research site had slowly come to realize that customer-oriented activities such as failure recovery service formed an important part of quality costs.[15] However, managers believed that the current quality system measured these activities with considerable error, and wanted to design new measurement systems. For example, one plant had launched an initiative to analyze the information in its customer complaint processing system to understand the sources of field failures and drivers of customer dissatisfaction. Our results suggest that the current quality measurement system, while not directly capturing lost sales, has significant forward-looking information about future sales.

Our results suggest potential reasons why Banker et al. (2000) did not find a significant association between the number of customer complaints and future sales. Their changes regression effectively has a single one-month lag, and thus cannot uncover delayed associations such as those documented here. Furthermore, their customer complaint metric simply tallies the number of failures, and, unlike external failure costs, does not capture failure *magnitude*.

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[14] This computation assumes that the four external failure cost regressors and the dependent variable are scaled by the same denominator, namely plant sales, ignoring the fact that the plant sales change with time. This is a reasonable assumption because, as shown in Table 2, the changes in plant sales over time are quite small.

[15] Hart et al. (1990) also argue that a "getting things right the first time" philosophy, although popular, is an incomplete characterization of quality. Since some failures will occur, a coherent service recovery program is an important dimension of quality.

Although the customers' overall perception of the product is the key determinant of future sales, these perceptions are too complex for a single measure to capture. For instance, defect rates and on-time deliveries are factory-initiated and may not fully reflect customer perceptions. In contrast, external failures are customer-initiated. However, these failure costs provide no information on the quality of the failure recovery service, or about customers who choose not to complain. Our results suggest that a collection of customer- and firm-initiated quality measures can overcome the shortcomings of individual quality measures in capturing the effect of product quality on future revenue.

Robustness Checks and Sensitivity Analyses

We conduct various specification checks for the third columns in Tables 4 and 5. First, the insignificant autocorrelations among the residuals at the 10 percent level suggest that the number of lags is sufficient.[16] Second, the insignificance of White's heteroskedasticity test at the 10 percent level suggests that our scaling choices are appropriate. As a further test to ensure that the results are not attributable to our scaling choice, we include $(1/\text{Sales}_{t-1})$ as a regressor. This regressor is not significant, and does not alter our inferences. To control for seasonal effects not captured by past sales, we include additional seasonal dummies. These dummies are jointly insignificant at the 10 percent level, and do not affect our inferences. Finally, tests with plant-level dummies indicate that these dummies are jointly insignificant, which further justifies pooling data across plants.

The tests in the previous section omitted prevention and appraisal expenditures, arguing that these expenditures do not directly measure product quality. However, these expenditures could significantly affect future sales if they affect product quality in ways not captured by failure costs and nonfinancial quality measures. Results including prevention and appraisal costs as regressors (Table 6, Column 1) indicate that these costs do not play a significant incremental role in explaining future sales after controlling for past sales and other quality measures. Similar results obtain with four lags, suggesting that failure costs and nonfinancial measures adequately capture the features of product quality that affect future sales.

Although lagged sales control for demand effects unrelated to past product quality, this control is incomplete. A plant's sales in a given period could be high if the firm as a whole enjoys increased demand. Thus, we control for contemporaneous sales changes averaged over all the firm's plants. This regressor is highly significant in Table 6, Column 2 (coefficient = 0.37, t-statistic = 2.83). Yet the effects on future sales of on-time deliveries and defect rates do not change, suggesting that our results are robust.[17] Similar results (untabulated) obtain for the four-lag case.

Our regressions use only the given plant's past quality and sales data as regressors, thus ignoring potential plant interactions. However, poor quality performance at one plant could cause a customer to reduce purchases at other plants. In our case, however, the plants are geographically widely dispersed (several are located in various countries outside the U.S.) and there is relatively little customer overlap. Since it is difficult to specify which

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[16] The lagged dependent variable in the regression precludes use of the Durbin-Watson test, so we use the Breusch-Godfrey test for autocorrelation instead (Greene 1997, 596–597).

[17] Another alternative for capturing contemporaneous demand shocks is to use the median of the contemporaneous sales change of all firms in the industry. However, an industry-wide sales regressor was not significant and did not change the results, probably because this measure captures industry-wide effects only, and not changes in the firm's market share relative to its competitors.

TABLE 6

OLS Regressions of Changes in Sales on One-Quarter Lagged Changes in Past Quality Measures and Additional Sales and Quality Regressors for 11 plants of a *Fortune 500* Manufacturing Firm over 18 Quarters

	Dependent Variable: $\Delta Sales_t$	
Regressors	*Regression (1)*	*Regression (2)*
Intercept	0.007 (0.64)	0.002 (0.19)
ΔExternal Failure$_{t-1}$	−1.504 (−0.74)	−1.818 (−0.93)
ΔInternal Failure$_{t-1}$	2.230 (1.51)	1.660 (1.17)
ΔDefect Rate$_{t-1}$	−0.00003** (−2.36)	−0.00002** (−2.27)
ΔOn-time$_{t-1}$	0.239*** (3.51)	0.231*** (3.54)
ΔPrevention$_{t-1}$	−1.634 (−0.47)	
ΔAppraisal$_{t-1}$	−0.515 (−0.16)	
ΔSales$_{t-1}$	−0.216** (−2.35)	−0.187** (−2.20)
ΔSales$_{t-4}$	0.483*** (6.85)	0.365*** (4.59)
ΔAverage Sales$_t$		0.372*** (2.83)
n	140	140
Adjusted R^2	0.37***	0.41***

***, **, * Statistically significant at the 1 percent, 5 percent, and 10 percent levels (two-tail), respectively.
t-statistics in parentheses.
We scale quarterly changes in prevention, appraisal, internal and external failure costs by the plant's sales level in the prior period. ΔAverage Sales$_t$ is the average of ΔSales$_t$ over all plants in the sample. Defect rate changes are changes in defect rates measured in parts per million.

plants are most likely to affect sales at a given plant, we average quality measures over all plants for each period, and regress sales changes on these measures. These measures are insignificant, suggesting that plant interactions do not confound our findings.

Caveats

Our research design is subject to several limitations. As with any panel data using multiple plants across time, instability across time, heterogeneity, or interactions among plants may affect our results. However, as noted earlier, the maturity of the firm and the geographical dispersion of its plants somewhat mitigate these concerns in our study. Second, we consider quality an exogenous choice. Although the past-period regressors are predetermined in the statistical sense, quality is endogenous if managerial compensation depends on (future) sales or if some unknown factor determines both sales and quality.[18] The lack of a specific economic model of quality choices to ground our empirical tests is another limitation. Finally, because we focus on one research site and one quality measurement system, readers should be cautious in generalizing from our results.

VI. CONCLUSIONS

Most cost-of-quality systems exclude intangibles such as revenues lost due to poor product quality. Therefore, critics argue that these financial measures do not capture the true costs or benefits of quality as thoroughly as nonfinancial quality measures. However, our study suggests that both financial quality measures, such as external product failure costs, and nonfinancial quality measures, such as defect rates and on-time deliveries, are significantly associated with future sales. One potential reason that both sets of measures are leading sales indicators is that external failure costs are customer-initiated measures, while defect rates and on-time deliveries are firm-initiated. These results imply that a *collection* of financial and nonfinancial quality measures best captures the effect of quality on future revenues.

Our results reinforce FASB's initiative to include operational quality measures such as defect rates, delivery speeds, and customer rejections in its comprehensive model of business reporting (FASB 1996). However, this risks competitors' gaining sensitive proprietary information about internal firm operations. A present value measure of the customer goodwill asset, based on estimated future revenue effects of these measures, is an attractive alternative that would inform investors without providing excessive information to competitors. Future research could examine the feasibility/effectiveness of such measures.

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[18] The exogeneity of such regressors is a standard assumption in the vast body of "information content" studies in accounting that examine the forward-looking nature of various measures such as fair values and nonfinancial measures without explicitly specifying how managers make the underlying asset choices.

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Intra-Group, Interstate Strategic Income Management for Tax, Financial Reporting, and Regulatory Purposes

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ABSTRACT: In this study we examine whether banks owned by interstate multibank holding companies coordinate their security gains and losses to manage their tax, earnings, and capital management objectives. Specifically, we examine whether the realization of security gains and losses is related to the objectives of the individual bank, the consolidated group, or both. We find subsidiary banks manage their gain realizations not only to reduce their own state taxes, but also strategically to reduce their consolidated groups' tax expense. Specifically, members of consolidated banking groups shift gain recognition to lower-taxed group members and away from higher-taxed group members. In addition, we find evidence suggesting that banks realize security gains and losses to manage both their own and their groups' financial statement earnings.

Keywords: *business taxation; earnings management; regulatory capital.*

Data Availability: *Data used in this study are available from public sources, as indicated in the text.*

I. INTRODUCTION

This study expands our understanding of cross-jurisdictional tax, earnings, and capital management by investigating how multistate banking organizations coordinate income recognition within their consolidated groups. We examine the association between group members' security gains and losses recognition and their state tax rates, earnings, and capital ratios.

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A bank may be independent or may be controlled by a bank holding company. Before the introduction of interstate branching during the mid-1990s, only bank holding companies that controlled multiple, individually incorporated subsidiary banks, each of which operated in a single state, could conduct interstate banking. We examine whether such consolidated banking groups reduced their joint state tax expenses by strategically designating which subsidiary banks recognize the taxable gains and losses resulting from rebalancing their consolidated investment portfolios. Similar to income shifting and manipulating accounting accruals, this strategic recognition of gains and losses reduced groups' tax expense by shifting gains to lower-taxed subsidiaries and away from higher-taxed subsidiaries.

Prior research has found limited evidence that bank holding companies manage income to avoid federal taxes.[1] This prior evidence might lead regulators to suspect that multistate affiliations and branching would not adversely affect high-tax states' abilities to collect taxes from banks. However, our evidence suggests this is not the case. Using a more powerful design that identifies income shifting within consolidated groups, we find that groups of affiliated banks do strategically realize security gains and losses to reduce their state tax burdens. We obtain this evidence from a period (1991–1992) before interstate branching was allowed. Our findings suggest that banks were using their limited flexibility to undermine state and local tax bases even before Congress enacted legislation to permit interstate branch banking. Our evidence lends legitimacy to concerns expressed in Congressional testimony that further relaxing banking regulations to allow interstate branching would exacerbate banks' opportunities to shift income between states, thereby helping banks avoid state and local taxation. If we could obtain data to examine bank holding companies' abilities to shift income among states after the implementation of interstate branching laws, then we expect the results would most likely be even more striking.

We incorporate estimates of state tax rates for group members to approximate each subsidiary's relative tax rate, and in so doing construct a more powerful test for detecting cross-jurisdictional tax planning than in previous studies that consider only the subsidiary's tax rate (e.g., Klassen et al. 1993; Harris 1993; Harris et al. 1993; Grubert et al. 1993; Jacob 1996; Collins and Shackelford 1997; Collins et al. 1998; Klassen and Shackelford 1998; Petroni and Shackelford 1999; Newberry and Dhaliwahl 1999). These previous studies implicitly define the relative tax rate with respect to the total sample rather than with respect to a subsidiary's group. Our design should reduce misclassifications of incentives that arise from consideration of only the individual subsidiary's tax rate and should capture the group's incentives more accurately.

The following example illustrates how our approach reduces this type of misclassification. Suppose that a consolidated group consists of two subsidiaries, each of which operate in one state. One subsidiary operates in a state with a 14 percent tax rate and the other in a state with an 11 percent tax rate. Further suppose that researchers include these subsidiaries in a study where the average tax rate for other subsidiaries in the sample is 6 percent. Studies that consider only a subsidiary's own tax rate would treat the subsidiary in the 11 percent state as relatively highly taxed, suggesting that the group would shift income away from this subsidiary. However, rather than shifting income away from this subsidiary, the group would actually shift income *into* this subsidiary because it has the lowest tax rate available to them. Our study more fully exploits the tax rate information by recognizing not only that 11 percent is relatively high compared to the overall sample, but, more importantly, that it is relatively low compared to the subsidiaries to or from which a group could shift income.

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[1] Previous research examines whether banks strategically time the realization of securities gains to minimize federal taxes on an intertemporal basis. We examine how controlled groups of banks allocate securities gains realizations across states to minimize their state taxes intertemporally. We do not examine physical transfers of property, since such transfers would occur at fair value and could not be used to shift income.

Our within-consolidated-group design also allows us to contribute to the stream of research that investigates the interaction of tax, earnings, and capital management incentives on the strategic timing of security gains and loss realizations (e.g., Scholes et al. 1990; Beatty et al. 1995; Collins et al. 1995; Collins et al. 1997; Beatty and Harris 1999; Mikhail 1999). We extend this research not only by focusing on state rather than federal tax rates, but also by examining earnings and capital management within the consolidated group. The greater variation in state relative to federal tax rates among profitable banks should yield more powerful tests of how taxes affect the management of security gains and losses. Our within-group design also allows us to investigate whether earnings and capital management is related to the subsidiary bank's or the group's objectives.

Our primary analysis uses the 458 subsidiary bank-years from 28 consolidated bank holding companies reporting data necessary for our analyses. Our results are consistent with consolidated groups strategically coordinating their security gains recognition to reduce the group's joint state tax expense. Specifically, after controlling for a member bank's own-state tax rate, we find that a particular subsidiary bank realizes more security gains when the state tax rates of other banks in its group are higher. We also find that banks' realizations of security gains and losses depend both on their own earnings and on the earnings of other banks in their group. This finding is consistent with consolidated groups coordinating earnings management to benefit the joint enterprise, and is also consistent with publicly traded holding companies being concerned with the information reported to their shareholders in the financial statements.

In the next section we provide background information on the regulation and taxation of interstate banking, and we discuss related research. In Section III we develop hypotheses. In Section IV we describe our research design. We discuss the sample selection criteria and descriptive statistics in Section V. Section VI contains our results. We discuss sensitivity and robustness analyses in Section VII, and provide concluding comments in Section VIII.

II. INSTITUTIONAL BACKGROUND AND RELATED RESEARCH

Regulation of Interstate Banking

Under the 1994 Riegle-Neal Interstate Banking and Branching Efficiency Act, interstate banking can be conducted via either branches or subsidiaries. Prior to this law, state law restricted interstate banking, virtually prohibiting interstate banking using subsidiaries until 1985 and prohibiting interstate branching until 1993. During the late 1980s many states began to permit multistate banking by allowing bank holding companies to control multiple banks, each of which operated in single state. As of January 1991, 32 states allowed national interstate banking and 16 other states allowed regional interstate banking.

Availability of Financial Information by State

Regulators require banks to provide aggregate financial information in the Consolidated Reports of Condition and Income (Call Report), but do not require them to separately report information for bank branches. Prior to interstate branching, banks reported state-specific income and taxes because they could not operate across state lines. After interstate branching, the lack of branch-level data makes it impossible to determine how much reported financial information is attributable to each state. Therefore, the regulatory environment prior to interstate branching provides a unique opportunity to examine how banks manage state taxes. In addition, it provides one of the few opportunities to examine the management of state taxes in any industry.

Ability to Shift Income Using Subsidiaries vs. Branches

Interstate branch banking provides bank holding companies the opportunity to reduce state tax expense by shifting income within a single subsidiary bank. Before interstate branching, however, bank holding companies could reduce state tax expense only by shifting income across subsidiary banks. If bank holding companies cannot manage their subsidiaries freely, then subsidiary banking is likely to result in fewer opportunities to use income shifting to reduce state tax expense than interstate branch banking.

Two types of regulatory restrictions likely impair holding companies' opportunities to manage their consolidated state tax burdens. First, each subsidiary bank, as well as the bank holding company, must meet separate regulatory requirements.[2] Second, regulatory restrictions on intersubsidiary transactions may further limit bank holding companies' opportunities to shift income.[3] The need to meet regulatory requirements at both the bank and bank holding company levels may limit the extent to which bank holding companies can use security gain and loss recognition to reduce state taxes. In addition, these restrictions limit the extent to which they can use other types of transactions to reduce state taxes.

However, because these limitations would not apply to the branches of a multistate bank, it is likely more difficult to minimize the state tax burden with a subsidiary organizational structure than with a branch structure. Marshall (1997) reports that Mark Olson, the director of regulatory consulting for Ernst & Young, argues that interstate branching provides many more options for arbitraging the state tax law. McCray (1991) expressed similar concerns when presenting an Advisory Commission on Intergovernmental Relations report on banking and state taxation during Congressional Hearings.[4] She argued that interstate branch banking would erode the tax bases of many states because that organizational structure increases banks' income-shifting opportunities. Therefore, our results should provide a conservative estimate of the extent to which banks can shift income to minimize state taxes after the implementation of interstate branching.

Research on Tax-Induced Income-Shifting Behaviors

Considerable research examines how companies use income shifting to manage their taxes. Although these studies generally find higher income reported in low-tax rather than in high-tax jurisdictions, most of these studies do not isolate the reason for this behavior or accurately compute its magnitude.

The existence of a single tax rate in each jurisdiction makes it difficult to control for other nontax jurisdictional effects. As a result, most studies have not attempted to separate the tax and nontax effects, although some have addressed this problem using a control/treatment design. For example, Harris et al. (1993) compare multinational and nonmultinational groups, and Harris (1993) uses a change in the U.S. tax law to separate tax and nontax effects. Petroni and Shackelford (1999) use a special "retaliatory" state tax feature that is unique to the insurance industry to compute a tax rate that depends not only on the state in which a subsidiary operates, but also on its state of incorporation. This enables Petroni and Shackelford (1999) to approximate the magnitude of state-tax-induced income shifting. Even so, they are able to estimate only a range of possible values because state tax rates are implicitly impounded in the price of local insurance.

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[2] Regulators rate each bank based on capital adequacy, asset quality, management, earnings, and liquidity. Banks that do not attain top ratings attract extra regulatory attention.

[3] Specifically, *the Bank Holding Company Supervision Manual* states in Section 2020.6 that "all fees for services rendered should be supported by written agreements that describe the services, the fees charged, and the method of allocating the fees among the subsidiaries" (Board of Governors of the Federal Reserve System). Asset sales must be conducted on market terms and are limited to 10 percent of a bank's capital with any one affiliate and to 20 percent of capital in total.

[4] McCray was legal counsel to the Multistate Tax Commission, principal author of Congress's sponsored report on banking and state taxation conducted by the Advisory Commission on Intergovernmental Relations, and professor of tax law at the University of Colorado.

In contrast, implicit taxes do not affect tests based on realizations of security gains and losses because security prices are determined in a national market and do not vary across states. Thus, we are able to provide more precise estimates of state-tax-induced income shifting because our findings are unlikely to be confounded by state-varying implicit taxes.

III. HYPOTHESIS DEVELOPMENT

Tax Management Hypothesis

A bank holding company doing business in more than one state has an incentive to shift income across states to minimize the combined state tax expense of its subsidiary banks. We expect that each bank subsidiary has incentives to reduce its own state tax expense and to reduce the tax expense of its consolidated group. Specifically, we predict that the amount of securities gains a subsidiary bank realizes will be positively related to the state tax rates of other subsidiaries in the consolidated group and will be negatively related to its own state tax rate.

Financial Statement Earnings Management Hypothesis

A subsidiary bank has several incentives to manage either its own financial-statement earnings, or those of its consolidated group, or both. First, accounting-based contracts designed to mitigate agency problems may encourage earnings management. Second, managers may have incentives to manage earnings to reduce information asymmetry. Third, banks may have an incentive to manage earnings because regulators analyze earnings relative to the earnings of peer banks when assessing the bank's financial soundness.[5]

A bank holding company is less likely than a subsidiary bank to enter into accounting-based contracts, because the primary business of a bank holding company is merely to own the banks that actually conduct the banking business. This suggests that an individual bank is more likely to use realized security gains and losses to manage its own earnings rather than those of its group to avoid violation of these contracts. In contrast, a desire to reduce information asymmetry between managers and shareholders is more likely to prompt a bank to manage the earnings of its group rather than its own earnings, since consolidated group earnings are publicly reported to shareholders. Finally, since bank regulation occurs at both the bank and holding-company levels, we expect earnings management to achieve regulatory goals to occur at both the individual bank level and the consolidated group level.

Capital Management Hypothesis

Previous research provides mixed evidence on whether bank holding companies use security gains and losses to manage capital. Scholes et al. (1990) find evidence consistent with capital management, while Collins et al. (1995) find only limited evidence of capital management, and Beatty et al. (1995) and Beatty and Harris (1999) find no evidence that bank holding companies use security gains and losses to manage capital. Because regulatory capital requirements are imposed at the bank level as well as at the holding-company level, it is possible that the limited support for the capital management hypothesis is due to prior research's focus on holding-company level rather than bank-level data. Therefore, we examine whether the realization of security gains and losses is related either to the individual bank's capital ratio, or to the average capital ratio of the other banks in its consolidated group, or both.

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[5] Beatty and Harris (1999) compare public vs. private banks to provide evidence about which of these three explanations are important determinants of earnings management. Based on their findings that public banks use security gain and loss recognition to engage in more earnings management than do private banks, and that these security gains and losses are more positively related to future earnings for public banks, they conclude that, at least in part, these public banks use earnings management to reduce information asymmetry.

IV. RESEARCH DESIGN

Measurement of State Tax Rates

The Advisory Commission on Intergovernmental Relations (1989) report indicates that in most states, the basis for taxing banks is grounded on administrative practice and regulation rather than on statutory authority. The marginal tax rate of banks with positive taxable income should depend on these administrative, regulatory, and statutory practices. State statutory tax rates are not easily determined and are often ambiguous. Petroni and Shackelford (1999) note that statutory rates are frequently misleading due to differing tax-base definitions and differing credit allowances across the states. Because of these difficulties, Petroni and Shackelford (1999) measure the marginal tax rate in each state using the median effective tax rate of subsidiaries operating in the state.[6]

Following Petroni and Shackelford (1999), we use regulatory filings to construct state-specific effective tax rates.[7] These rates are based on all banks operating in the state, including banks that are owned by multistate holding companies. Most of the research examining the effects of federal taxes does not use effective tax rates but instead uses a dichotomous variable design—0 for nontaxable, net operating loss (NOL) firms, and 1 otherwise. Our use of a median state tax rate based on the many banks operating in each state provides a more refined measure than a dichotomous NOL variable, which should reduce measurement error while also capturing the wide variation of state tax rates. In sensitivity tests we allow the estimated coefficient on the tax rate to differ for banks with nonpositive income and we substitute banks' individual effective state tax rates for our tax rate measures.

Models Estimated

To test our tax, earnings, and capital management hypotheses we estimate the following three equations:

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[6] Allocation of income and the use of direct vs. franchise taxes on income give rise to two other potential complications in determining tax rates. However, neither of these issues is likely to affect the taxation of gains and losses on sales of securities. During our sample period, states generally taxed banks on an individual, stand-alone basis. Although states used specific location rules for identifiable income sources, security gains were not subject to allocation since they were associated with the bank holding the security. Security gains and losses are also not treated differently in states that tax banks via a franchise tax based on income vs. those that tax income directly.

[7] We would prefer to use marginal tax rates, but the wide variety of state tax bases and administrative and regulatory sources of tax law makes these data unavailable. We proxy for marginal tax rates with median effective, or average, tax rates, as do Petroni and Shackelford (1999). This yields appropriate proxies if higher marginal rates cause higher effective rates, which should generally be the case when tax rates are progressive.

$$\begin{aligned}\text{RealizedGains}_{i,t} = {} & \beta_0 + \beta_1\text{TaxRate}_{i,t} + \beta_2\text{PretaxIncome}_{i,t} \\ & + \beta_3\mu_{j\neq i}\text{PretaxIncome}_{i,t} + \beta_4\text{Capital}_{i,t} + \beta_5\mu_{j\neq i}\text{Capital}_{i,t} \\ & + \beta_6\text{TotalGains}_{i,t} + \beta_7\mu_{j\neq i}\text{TotalGains}_{i,t} + \beta_8\ \text{Ln[Assets]}_{i,t} \\ & + \beta_9 D_{1991} + \sum_{k=1}^{N_{state}-1} \beta_{10,k} D_{state_i} + \sum_{l=1}^{N_{group}-1} \beta_{11,l} D_{group_l} + \varepsilon_{i,t}; \quad (1)\end{aligned}$$

$$\begin{aligned}\text{RealizedGains}_{i,t} = {} & \beta_0 + \beta_1\text{TaxRate}_{i,t} + \beta_{1a}\mu_{j\neq i}\ \text{TaxRate}_{i,t} + \beta_2\text{PretaxIncome}_{i,t} \\ & + \beta_3\mu_{j\neq i}\text{PretaxIncome}_{i,t} + \beta_4\text{Capital}_{i,t} + \beta_5\mu_{j\neq i}\text{Capital}_{i,t} \\ & + \beta_6\text{TotalGains}_{i,t} + \beta_7\mu_{j\neq i}\text{TotalGains}_{i,t} + \beta_8\ \text{Ln[Assets]}_{i,t} \\ & + \beta_9 D_{1991} + \sum_{k=1}^{N_{state}-1} \beta_{10,k} D_{state_i} + \sum_{l=1}^{N_{group}-1} \beta_{11,l} D_{group_l} + \varepsilon_{i,t}; \quad (2)\end{aligned}$$

$$\begin{aligned}\text{RealizedGains}_{i,t} = {} & \beta_0 + \beta_1\text{TaxRate}_{i,t} + \beta_{1b}\text{TaxRate}_{High\,i,t} + \beta_{1c}\text{TaxRate}_{Low\,i,t} \\ & + \beta_2\text{PretaxIncome}_{i,t} + \beta_3\mu_{j\neq i}\text{PretaxIncome}_{i,t} + \beta_4\text{Capital}_{i,t} \\ & + \beta_5\mu_{j\neq i}\text{Capital}_{i,t} + \beta_6\text{TotalGains}_{i,t} + \beta_7\mu_{j\neq i}\text{TotalGains}_{i,t} \\ & + \beta_8\ \text{Ln[Assets]}_{i,t} + \beta_9 D_{1991} + \sum_{k=1}^{N_{state}-1} \beta_{10,k} D_{state_i} \\ & + \sum_{l=1}^{N_{group}1} \beta_{11,l} D_{group_l} + \varepsilon_{i,t}; \quad (3)\end{aligned}$$

where subscripts indicate bank i and year t. The variables are defined as follows:

RealizedGains = realized security gains, scaled by total assets;
TaxRate = the median effective state tax rate of all banks operating in the state that report state income tax expense;
$\mu_{j\neq i}$TaxRate = the average TaxRate of all banks in the consolidated group other than bank i;
TaxRate_{High} = the highest TaxRate of any bank in the consolidated group;
TaxRate_{Low} = the lowest TaxRate of any bank in the consolidated group;
PretaxIncome = pretax income, before security gains, scaled by total assets;
$\mu_{j\neq i}$PretaxIncome = the average PretaxIncome of all banks in the consolidated group other than bank i;
Capital = reported Tier 1 capital (which includes common equity, qualifying preferred equity, minority interests, less disallowed intangibles) before realized security gains, scaled by total assets;
$\mu_{j\neq i}$Capital = the average Capital of all banks in the consolidated group other than bank i;
TotalGains = total security gains available to be realized, computed by adding back the realized security gains to the ending fair market value of securities less their cost, scaled by total assets;
$\mu_{j\neq i}$TotalGains = the average TotalGains of all banks in the consolidated group other than bank i;
Ln(Assets) = the natural log of total assets;
D_{state} = equals 1 if the bank operates in the state, and 0 otherwise;
D_{1991} = equals 1 if year is 1991, else 0;
D_{group} = equals 1 if the bank is a member of the group, else 0;
N_{state} = equals the number of states represented in the sample; and
N_{group} = equals the number of bank holding companies represented in the sample.

Equations (1)–(3) reflect successively finer analyses. Equation (1) focuses on a bank's own-state tax rate. We expect β_1 to be negative if banks realize lower securities gains when their state tax rates are higher. Equations (2) and (3) include measures of the state tax rates of other members of a bank's consolidated group. In equation (2) we consider the effect of the mean tax rate of the other members of a bank's group. We predict that β_{1a} will be positive, since the bank's relative tax cost will be lower when the average tax rate of other group members is higher. In Equation (3) we further disaggregate the effect of other group members' tax costs by including separate variables for the highest and lowest tax rates in the group. We predict that both β_{1b} and β_{1c} will be positive, since a bank's relative tax cost is lower when the highest and lowest tax rates of the group are higher.

We use two variables to examine our earnings-management hypotheses. We include a bank's earnings before security gains to determine whether banks manage their own income. Based on the results of prior research suggesting that banks use security gains and losses to smooth reported earnings, we expect β_2 to be negative. We also include the average income before security gains of other group members. If banks use their own security gains to manage the group's income, then we also expect β_3 to be negative.

We also use two variables to examine our capital-management hypotheses. First, we include a bank's own capital. We expect β_4 to be negative if banks use security gain and loss realizations to manage their own regulatory capital, since realized security gains increase regulatory capital and banks with low capital have more incentive to increase this ratio. In addition, we include a regulatory capital measure based on the other members of a bank's group. A negative β_5 coefficient would suggest a bank uses its gain realizations to manage its group's regulatory capital.

Equations (1)–(3) include several variables to control for other possible influences on a bank's realization of security gains and losses. We include both the bank's total potential security gains and the total available security gains of other members of a bank's consolidated group. We predict that β_6 will be positive because we expect a bank's actual realizations to be positively correlated with its available potential gains.[8] We have no prediction for the sign of β_7, since we have no reason to expect that the bank's realized gains will be related to the gains available to other group members. We include the log of assets to control for possible size effects, which is captured by β_8.

The analysis includes additional econometric controls: (1) an indicator variable for the year 1991; (2) separate indicator variables for each state in our sample; and (3) indicator variables for the consolidated group to which the bank belongs. These controls should attenuate possible influences of correlated errors on our significance tests and should increase the reliability of our inferences.

V. SAMPLE AND DATA

Sample Selection

We draw our sample from all commercial banks that filed a Call Report during 1991–1992. We focus on 1991 and 1992 because: (1) this is the period prior to the inception of interstate branching in which the greatest number of states allowed interstate multibank holding companies; and (2) banks' regulatory, tax, and accounting environments are relatively constant throughout this period.[9] We limit our sample to banks held by publicly traded bank holding companies because Beatty and Harris (1999) document differences in earnings and tax management practices for public vs. private banks.

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[8] The sensitivity analysis section shows that an alternative specification where the dependent variable is the ratio of realized securities gains to total available securities gains leads to identical inferences.

[9] Calculation of our effective state tax rate becomes complicated after 1992 because some states began to allow interstate branching in that year. In addition, the adoption of SFAS No. 115 during 1993 may have influenced banks' securities gain and loss realizations. Regulatory capital requirements changed during 1990 with the implementation of the risk-based capital requirements, which became effective as of December 31, 1990.

Our primary analysis further excludes banks from the sample if any of the bank's group members operates in a state that does not tax income from U.S. obligations. We place this restriction on our sample because estimates of the effective tax rates for states that do not tax these obligations are less reliable. For example, excluding the interest on these securities from taxable income results in a median observed effective state tax rate for our banks of zero for 19 of the 24 states (including the District of Columbia) that do not tax U.S. obligations. However, profitable banks in these states may not face a zero marginal tax rate on taxable income. Thus, our primary analysis excludes groups with member banks in any of these states because their estimated effective tax rates would be poor proxies for the banks' marginal tax rates. We report the sensitivity of our results to this restriction in the sensitivity analysis section.

Table 1 summarizes the states that tax U.S. obligations and the number of bank-years in our total and primary samples by state of operation. Our primary sample includes observations in only 21 of the 27 states that tax U.S. obligations. In the other 6 taxing states there are no banks belonging to groups where all the group members operate in taxable states.

Panel A of Table 2 provides information about the bank holding companies in our full sample. There are a total of 125 publicly traded interstate multibank holding companies in our sample period. The median number of banks held by these consolidated groups is 4 with a low of 2 and a high of 78. The median group includes banks operating in 3 states with a low of 2 and a high of 12.

Panel B of Table 2 shows that our primary sample includes 28 bank holding companies. The median number of banks included in these consolidated groups is 3, and the number ranges from 2 to 41. The median group includes banks operating in 2 states; the largest number of states in which any group operates is 6.

The advantage of considering within-group variation in tax rates, rather than considering only a subsidiary's own tax rate relative to the total sample, exists only if not all banks operate in all states. Our sample banks do business in widely differing combinations of states, which suggests that our within-group design may be more powerful than tests used in prior studies.

Descriptive Statistics

Table 3 presents descriptive statistics about the multistate bank-years in our primary sample. The mean TaxRate is 3.58 percent, with a high of 12.28 percent and a low of 0 percent. The mean $\text{TaxRate}_{\text{High}}$ is 6.07 percent and the mean $\text{TaxRate}_{\text{Low}}$ is 0.07 percent. This suggests that, on average, a bank holding company could save approximately 6 percent of realized income by shifting security sales from the bank with the highest tax rate to the bank with the lowest rate.

Table 4 shows the correlations among the variables. Not surprisingly, the correlations between a bank's own tax rate and those of other banks in its group are high. Specifically, the correlations: (1) between TaxRate and $\mu_{j\neq i}$TaxRate is 56.2 percent; (2) between TaxRate and $\text{TaxRate}_{\text{High}}$ is 57.42 percent; and (3) between TaxRate and $\text{TaxRate}_{\text{Low}}$ is 49.08 percent. We also find fairly high correlations among the other characteristics of banks operating in a consolidated group. For example, the correlation between PretaxIncome and $\mu_{j\neq i}$PretaxIncome is 33.60 percent, and the correlation between Capital and $\mu_{j\neq i}$Capital is 36.93 percent. Though simple correlations between the regressors sometimes exceed 30 percent, sensitivity analyses reported later reveal that multicollinearity has little or no effect on our results.

TABLE 1
Taxable Status of Income on U.S. Obligations by State of Operation of Bank Subsidiaries of Interstate Multibank Holding Companies—for Total Sample[a] of 2,279 Bank-Years and Primary Sample[b] of 458 Bank-Years

State	*Taxable*	*Total Bank-Years*	*Primary Bank-Years*	*State*	*Taxable*	*Total Bank-Years*	*Primary Bank-Years*
Alabama	Yes	65	59	Montana	Yes	18	0
Alaska	No	6	0	Nebraska	No	17	0
Arkansas	No	32	0	Nevada	No	15	0
Arizona	No	27	0	New Hampshire	No	9	0
California	Yes	33	3	New Jersey	Yes	23	0
Colorado	No	108	0	New Mexico	No	16	0
Connecticut	Yes	13	6	New York	Yes	36	6
D.C.	No	14	0	North Carolina	Yes	17	15
Delaware	Yes	41	10	North Dakota	Yes	14	9
Florida	Yes	146	128	Ohio	Yes	119	7
Georgia	Yes	44	40	Oklahoma	No	7	0
Hawaii	Yes	2	0	Oregon	No	19	0
Idaho	No	14	0	Pennsylvania	No	53	0
Illinois	No	175	0	Rhode Island	No	4	0
Indiana	Yes	139	16	South Carolina	Yes	19	15
Iowa	Yes	47	2	South Dakota	Yes	22	12
Kansas	Yes	6	0	Tennessee	Yes	64	20
Kentucky	No	70	0	Texas	No	68	0
Louisiana	No	5	0	Utah	Yes	10	0
Maine	Yes	12	0	Vermont	No	4	0
Maryland	Yes	63	39	Virginia	Yes	66	39
Massachusetts	Yes	20	12	Washington	No	21	0
Michigan	No	92	0	West Virginia	No	39	0
Minnesota	Yes	59	17	Wisconsin	Yes	179	1
Mississippi	No	11	0	Wyoming	No	16	0
Missouri	Yes	160	2	Total Bank-Years		2,279	458

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[a] The total sample of bank-years is all bank-years during 1991 and 1992 for banks that are owned by publicly traded, interstate multibank holding companies.

[b] The primary sample of bank-years excludes bank-years from the total sample if any of the bank's group members operates in a state that does not tax income from U.S. obligations.

TABLE 2
Number of Subsidiary Banks and States of Operations for 125 Multistate Bank Holding Companies in Total Sample[a] and 28 Multistate Bank Holding Companies in Primary Sample[b]

Variable	*Mean*	*Median*	*Minimum*	*Maximum*
Panel A: Total Sample of 125 Bank Holding Companies				
Number of Subsidiary Banks	9	4	2	78
Number of States Operated In	3.4	3	2	12
$\text{TaxRate}_{\text{High}} - \text{TaxRate}_{\text{Low}}$[c]	0.0580	0.0578	0	0.1054
Panel B: Primary Sample of 28 Bank Holding Companies				
Number of Subsidiary Banks	8	3	2	41
Number of States Operated In	2.6	2	2	6
$\text{TaxRate}_{\text{High}} - \text{TaxRate}_{\text{Low}}$[c]	0.0546	0.0547	0.0165	0.1054

[a] The total sample of bank holding companies includes all publicly traded, interstate multiple bank holding companies operating during either 1991 or 1992.

[b] The primary sample of bank holding companies excludes from the total sample bank holding companies that have a subsidiary operating in a state that does not tax income from U.S. obligations.

[c] $\text{TaxRate}_{\text{High}} - \text{TaxRate}_{\text{Low}}$ = the highest TaxRate of any bank in the consolidated group minus the lowest TaxRate of any bank in the consolidated group, where TaxRate is the median effective state tax rate of all banks operating in the state that report state income tax expense.

VI. RESULTS

The results from estimating equations (1)–(3) appear in Table 5.[10] The coefficient on TaxRate, β_1, is significantly negative ($\alpha < 0.10$; one-tailed) in all three models. The coefficient of −0.0219 on TaxRate in model (1) implies that a one-percentage-point higher effective state tax rate leads to a 0.000219 lower realized security-gains-to-assets ratio. Given a mean pretax income-to-assets ratio of 0.0139, banks respond to a one-percentage-point higher TaxRate by reducing reported pretax income by 1.6 percent (0.000219/0.0139). This suggests that banks' realizations of security gains and losses are sensitive to their own tax rates, but does not indicate whether they take advantage of differences in tax rates across their consolidated groups.

The significantly positive coefficient ($\alpha < 0.01$) on $\mu_{j\neq i}$TaxRate in model (2), β_{1a}, suggests that, on average banks realize more gains from securities sales when the other members' state tax rates are greater. The coefficient of 0.039 indicates, holding constant a bank's own tax rate, that a one-percentage-point higher average tax rate of the other banks in the group results in a bank reporting pretax income that is 2.8 percent (0.00039/0.0139) higher.

In sum, a bank reports significantly lower security gains when its own tax rate is higher, holding constant the average tax rate for the other members of its group, and a bank reports significantly higher security gains when the average tax rate for the other members of its group is higher, holding constant its own tax rate. However, when the difference in the tax rates of the bank and the other members of its controlled group is unchanged, there is no

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[10] For compactness of presentation, we suppress the estimates of the coefficients on the state and group indicator variables.

TABLE 3
Descriptive Statistics for the Regression Variables for the Primary Sample of 458 Multistate Bank-Years

Variable	*Mean*	*Standard Deviation*	*Minimum*	*Maximum*
RealizedGains	0.0005	0.0014	−0.0029	0.0152
TaxRate	0.0358	0.0305	0.0000	0.1228
$\mu_{j\neq i}$TaxRate	0.0358	0.0232	0.0000	0.1228
TaxRate$_{High}$	0.0607	0.0249	0.0188	0.1228
TaxRate$_{Low}$	0.0007	0.0165	0.0000	0.0836
PretaxIncome	0.0139	0.0180	−0.1081	0.1331
$\mu_{j\neq i}$PretaxIncome	0.0139	0.0112	−0.1045	0.0526
Capital	0.0912	0.0570	0.0241	0.8283
$\mu_{j\neq i}$Capital	0.0912	0.0350	0.0241	0.3761
TotalGains	0.0080	0.0069	−0.0033	0.0655
$\mu_{j\neq i}$TotalGains	0.0080	0.0043	0.0000	0.0434
Ln[Assets]	12.6003	1.6949	9.4466	18.1847

The variables are defined as follows:

RealizedGains = realized security gains, scaled by total assets;
TaxRate = the median effective state tax rate of all banks operating in the state that report state income tax expense;
$\mu_{j\neq i}$TaxRate = the average TaxRate of all banks in the consolidated group other than bank i;
TaxRate$_{High}$ = the highest TaxRate of any bank in the consolidated group;
TaxRate$_{Low}$ = the lowest TaxRate of any bank in the consolidated group;
PretaxIncome = pretax income, before security gains, scaled by total assets;
$\mu_{j\neq i}$PretaxIncome = the average PretaxIncome of all banks in the consolidated group other than bank i;
Capital = reported Tier 1 capital (which includes common equity, qualifying preferred equity, minority interests, less disallowed intangibles) before realized security gains, scaled by total assets;
$\mu_{j\neq i}$Capital = the average Capital of all banks in the consolidated group other than bank i;
TotalGains = total security gains available to be realized, computed by adding back the realized security gains to the ending fair market value of securities less their cost, scaled by total assets;
$\mu_{j\neq i}$TotalGains = the average TotalGains of all banks in the consolidated group other than bank i; and
Ln(Assets) = the natural log of total assets.

significant change in the security gains realized, (i.e., the sum of the coefficients on TaxRate and $\mu_{j\neq i}$TaxRate is statistically insignificant). These results suggest that banks are not merely responding to the level of their own state tax rates, but to the differences between their tax rate and the rates of the other members of their consolidated groups, consistent with our tax management hypothesis.

The results from model (3) also indicate that banks are responding to the relative tax rates of their consolidated groups. β_{1c} is significantly positive ($\alpha < 0.01$), indicating that banks realize more securities gains when the lowest tax rate for other banks in the same group is higher. The coefficient of 0.0601 on TaxRate$_{Low}$ indicates that a one-percentage-point higher rate, holding constant a bank's own tax rate, results in 4.3 percent (0.0601

TABLE 4

Pearson Correlations (p-values) for the Regression Variables for the Primary Sample of 458 Multistate Bank-Years

	TaxRate	$\mu_{j\neq i}$*Tax Rate*	*Tax Rate*$_{High}$	*Tax Rate*$_{Low}$	*Pretax Income*	$\mu_{j\neq i}$*Pretax Income*	*Capital*	$\mu_{j\neq i}$*Capital*	*Total Gains*	$\mu_{j\neq i}$*Total Gains*
$\mu_{j\neq i}$TaxRate	0.5620 (0.0001)	1.0000 (0.0)								
TaxRate$_{High}$	0.5742 (0.0001)	0.7370 (0.0001)	1.0000 (0.0)							
TaxRate$_{Low}$	0.4908 (0.0001)	0.6290 (0.0001)	0.5654 (0.0001)	1.0000 (0.0)						
PretaxIncome	0.0628 (0.1795)	−0.04246 (0.3646)	0.0045 (0.9234)	0.0627 (0.1806)	1.0000 (0.0)					
$\mu_{j\neq i}$PretaxIncome	−0.0385 (0.4115)	0.0321 (0.4936)	0.0068 (0.8840)	0.0952 (0.0418)	0.3360 (0.0001)	1.0000 (0.0)				
Capital	0.1840 (0.0001)	0.1141 (0.0145)	0.1583 (0.0007)	0.0699 (0.1347)	0.4396 (0.0001)	0.2167 (0.0001)	1.0000 (0.0)			
$\mu_{j\neq i}$Capital	0.1768 (0.0001)	0.2248 (0.0001)	0.2577 (0.0001)	0.1139 (0.0147)	0.2322 (0.0001)	0.5052 (0.0001)	0.3693 (0.0001)	1.0000 (0.0)		
TotalGains	−0.0100 (0.8346)	0.0545 (0.2449)	0.1351 (0.0048)	0.0455 (0.3317)	0.0288 (0.5394)	−0.0158 (0.7363)	−0.0745 (0.1116)	−0.0698 (0.1360)	1.0000 (0.0)	
$\mu_{j\neq i}$TotalGains	0.0537 (0.2516)	0.0523 (0.2642)	0.2084 (0.0001)	0.0721 (0.1236)	−0.0165 (0.7252)	0.1692 (0.0003)	−0.0680 (0.1465)	−0.1293 (0.0056)	0.3827 (0.0001)	1.0000 (0.0)
Ln(Assets)	−0.0950 (0.0421)	−0.1693 (0.0003)	−0.1945 (0.0001)	−0.0842 (0.0719)	−0.0356 (0.4468)	−0.0063 (0.8934)	−0.1609 (0.0005)	0.1692 (0.0003)	0.0001 (0.9991)	−0.1293 (0.0056)

(Continued on next page)

TABLE 4 (Continued)

The variables are defined as follows:

RealizedGains = realized security gains, scaled by total assets;

TaxRate = the median effective state tax rate of all banks operating in the state that report state income tax expense;

$\mu_{j \neq i}$TaxRate = the average TaxRate of all banks in the consolidated group other than bank i;

$\text{TaxRate}_{\text{High}}$ = the highest TaxRate of any bank in the consolidated group;

$\text{TaxRate}_{\text{Low}}$ = the lowest TaxRate of any bank in the consolidated group;

PretaxIncome = pretax income, before security gains, scaled by total assets;

$\mu_{j \neq i}$PretaxIncome = the average PretaxIncome of all banks in the consolidated group other than bank i;

Capital = reported Tier 1 capital (which includes common equity, qualifying preferred equity, minority interests, less disallowed intangibles) before realized security gains, scaled by total assets;

$\mu_{j \neq i}$Capital = the average Capital of all banks in the consolidated group other than bank i;

TotalGains = total security gains available to be realized, computed by adding back the realized security gains to the ending fair market value of securities less their cost, scaled by total assets;

$\mu_{j \neq i}$TotalGains = the average TotalGains of all banks in the consolidated group other than bank i; and

Ln(Assets) = the natural log of total assets.

× 0.01/0.0139) greater reported pretax income. The combined coefficient on TaxRate and $\text{TaxRate}_{\text{Low}}$ is −0.0331 (−0.0932 + 0.0601) and is statistically significant ($\alpha < 0.01$). This indicates that when both the bank's tax rate and the lowest rate in the group are higher it is less attractive to shift gain realization to the bank. These results further support our tax management hypothesis. The coefficient on $\text{TaxRate}_{\text{High}}$, β_{1b}, and the combined coefficient on TaxRate, $\text{TaxRate}_{\text{Low}}$, and $\text{TaxRate}_{\text{High}}$ are statistically insignificant.

TABLE 5

Results for Regression of Realized Security Gains and Losses on Tax, Earnings, Capital, and Control Variables (Including Fixed State, Group, and Year Effects)

	Expected Sign	*Model (1) Estimated Coefficients (t-statistics)*	*Model (2) Estimated Coefficients (t-statistics)*	*Model (3) Estimated Coefficients (t-statistics)*
β_0 Intercept	+/−	−0.0010 (−0.483)	−0.0045 (−1.958)	−0.0043 (−1.026)
β_1 TaxRate	−	−0.0219* (−1.565)	−0.0256** (−1.849)	−0.0932*** (−4.799)
β_{1a} $\mu_{j\neq i}$TaxRate	+		0.0390*** (3.103)	
β_{1b} $\text{TaxRate}_{\text{High}}$	+			0.0575 (1.196)
β_{1c} $\text{TaxRate}_{\text{Low}}$	+			0.0601*** (3.740)
β_2 PretaxIncome	−	−0.0103*** (−2.145)	−0.0122*** (−2.546)	−0.0140*** (−2.945)
β_3 $\mu_{j\neq i}$PretaxIncome	−	−0.0132 (−1.121)	−0.0285*** (−2.250)	−0.0305*** (−2.525)
β_4 Capital	−	0.0007 (0.239)	0.0009 (0.280)	0.0007 (0.246)
β_5 $\mu_{j\neq i}$Capital	−	0.0117 (2.963)	0.0112 (2.853)	0.0102 (2.063)
β_6 TotalGains	+	0.0191* (1.608)	0.0200** (1.697)	0.0200** (1.716)
β_7 $\mu_{j\neq i}$TotalGains	+/−	−0.0433 (−1.113)	−0.0147 (−0.370)	−0.0273 (−0.706)
β_8 Ln(Assets)	+/−	0.0002*** (3.829)	0.0002*** (3.459)	0.0002*** (3.596)
β_9 D_{1991}	+/−	−0.0002 (−0.772)	−0.0003 (−1.245)	−0.0003 (1.237)
Sample size		458	458	458
Adjusted R^2		0.3139	0.3282	0.3528

(*Continued on next page*)

TABLE 5 (Continued)

*, **, *** Indicates a significance level at 10 percent level or less, 5 percent level or less, and 1 percent level or less, respectively, determined with a one-tailed test where a directional hypothesis exists.

We do not report the coefficient estimates on the state indicator variables and the group dummy control indicator variables because of space limitations.

The variables are defined as follows:

RealizedGains = realized security gains, scaled by total assets;
TaxRate = the median effective state tax rate of all banks operating in the state that report state income tax expense;
$\mu_{j\neq i}$TaxRate = the average TaxRate of all banks in the consolidated group other than bank i;
$\text{TaxRate}_{\text{High}}$ = the highest TaxRate of any bank in the consolidated group;
$\text{TaxRate}_{\text{Low}}$ = the lowest TaxRate of any bank in the consolidated group;
PretaxIncome = pretax income, before security gains, scaled by total assets;
$\mu_{j\neq i}$PretaxIncome = the average PretaxIncome of all banks in the consolidated group other than bank i;
Capital = reported Tier 1 capital (which includes common equity, qualifying preferred equity, minority interests, less disallowed intangibles) before realized security gains, scaled by total assets;
$\mu_{j\neq i}$Capital = the average Capital of all banks in the consolidated group other than bank i;
TotalGains = total security gains available to be realized, computed by adding back the realized security gains to the ending fair market value of securities less their cost, scaled by total assets;
$\mu_{j\neq i}$TotalGains = the average TotalGains of all banks in the consolidated group other than bank i;
Ln(Assets) = the natural log of total assets; and
D_{1991} = equals 1 if year is 1991, else 0.

The importance of measuring a bank's tax rate relative to the tax rates for other group members can be seen by the difference in the magnitude of the coefficient on that variable in model (3) vs. model (1). After controlling for both the lowest and highest tax rate for any group member, the coefficient on the bank's tax rate is −0.0932, which is more than 4 times greater than the estimate in model (1). This indicates that the inclusion of within-group tax measures is important when assessing the effect of a tax rate change on the amount of income realized.[11]

The coefficient on a bank's earnings before security gains is significantly negative ($\alpha < 0.01$), suggesting that the lower the bank's earnings before security gains, the more security gains the bank realizes. That is, our results suggest that banks use security gains and losses to manage their own earnings. In addition, the coefficient on the average earnings of a bank's other group members is significantly negative ($\alpha < 0.01$) in both Equations (2) and (3). This is consistent with banks using security gains and losses to manage not only their own earnings, but also the earnings of their group, by realizing more security gains when the group's earnings are low, and conversely. These results support our earnings management hypothesis.

The coefficients on the bank's own capital ratio and other group members' capital ratios, β_4 and β_5, are insignificant in all three models. Thus, we find no evidence that banks use security gains and losses to manage capital. These results do not support our capital management hypothesis, but they are consistent with Beatty et al. (1995) and Beatty and Harris (1999). The coefficient on total potential security gains is positive, consistent with banks recognizing more gains when they have more gains available in their investment portfolios.

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[11] This increase in the magnitude of the coefficient occurs in both our primary and total samples. Since our multicollinearity diagnostics indicate that the proportion of variance of each of our variables of interest is well below the recommended cut-off of 50 percent in the total sample, it does not appear that this result is driven by a multicollinearity problem.

VII. SENSITIVITY AND ROBUSTNESS ANALYSES

Sensitivity to Exclusion of Banks in States That Do Not Tax U.S. Obligations

Restricting our primary sample to holding companies that operate exclusively in states that tax U.S. obligations reduces our sample to approximately 20 percent of the total population of publicly traded interstate multibank holding companies. In addition, our primary sample includes banks operating in only 21 states. We therefore examine the sensitivity of our results by re-estimating Equations (1)–(3) for the total sample. The magnitudes of the tax rate variables' coefficients drop somewhat, as would be expected when introducing additional measurement error. However, the significance of the results remains largely unchanged, except that in Equation (2) the coefficient on the average tax rate for other banks in the group becomes insignificant, and in Equation (3) the coefficient on the highest tax rate of any bank in the consolidated group becomes significantly positive ($\alpha < 0.01$). The reduction in the statistical significance of the average tax rate variable is not surprising. The average tax rate is most affected by the increased measurement error in the tax rate because it is an average of rates from taxable and nontaxable states. In contrast, the highest tax rate variable is least likely to be affected by this measurement error, since banks with the highest tax rates are more likely to be in states that tax U.S. obligations. The increased significance of this variable is likely due to greater power of the test from an increased sample size.

Sensitivity to Excluding Private Banks

Our tax results do not change when we estimate equations (1)–(3) including both public and private banks. Given that Beatty and Harris (1999) found only private banks sensitive to federal taxes, this result suggests two nonmutually exclusive possibilities. The first is that public banks find greater value in managing state taxes than federal taxes. The second is that strategically shifting security gains and losses from one state to another may be less costly than managing federal taxes because state shifting does not distort the group's reported financial earnings other than by reducing state taxes.

Sensitivity to Measurement of Effective State-Tax Rates

We also examine our results' sensitivity to the measurement of effective state tax rates. Our measure of the state tax rate does not take into account potential variation in tax rates across banks within a state. Specifically, an individual bank may face a lower state tax rate if it has nonpositive income. In our sample, less than 10 percent of the bank-years have nonpositive income. We address this potential limitation in our measurement of effective state tax rates by allowing the estimated coefficients on our tax variables in equations (1)–(3) to differ for banks with nonpositive income. Specifically, we allow the coefficients to differ (1) on the bank's own tax rate for banks with nonpositive income; (2) on the average tax rate of any bank in the consolidated group and on the low tax rate if any member of the group has nonpositive income; and (3) on the highest tax rate if all of the banks in the highest-rate state have nonpositive income. Our inferences are unchanged by allowing these coefficients to vary.

We also substitute banks' individual effective state tax rates for our measures of state tax rates. We compute individual bank effective tax rates by dividing the bank's state tax expense by the pretax financial-statement income. We find no significant effects for any of our tax variables in any of our models using these bank-specific tax rates.[12] This lack of significant effects for both the bank's own tax rate and the within-group rates is consistent with substantial measurement error in this variable. The argument in previous research that individual firms' effective tax rates measure the marginal tax rate with substantial error is consistent with the implausible variation we observe in the individual banks' estimated effective tax rates. Specifically, for sample bank-years with positive income, the effective tax rate varies from −6 percent to 47 percent, while for those with nonpositive income, the bank-specific estimated effective tax rate varies from −7 percent to 63 percent. This wide range of observed tax rates, combined with the observation of both positive and negative effective tax rates regardless of the sign of reported income, illustrates the noise in bank-specific effective tax rates as estimates of marginal tax rates.

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[12] This lack of significance remains after limiting the sample to firm-years with positive income.

Graham (1996), Plesko (1999), and Shackelford and Shevlin (2001) provide several explanations why the observed *ex post* individual bank state tax rates might differ from the *ex ante* expected rate. For example, differences between the individual effective tax rates and the marginal tax rate may arise because the denominator of the effective tax rate is book income rather than taxable income. Inclusion of nontaxable items in book income can cause the effective tax rate to be either higher or lower than the rate actually applied to taxable income. For example, inclusion of tax-exempt income in book income understates estimated marginal tax rates; and exclusion from book income of nontax deductible goodwill amortization overstates estimated marginal tax rates. When book income is close to zero these discrepancies will be magnified, and indeed nontaxable items may even cause book and taxable income—and thus the effective and marginal tax rates—to have the opposite signs.

A second reason for the observed *ex post* individual bank state tax rates to differ from the *ex ante* expected rate arises because a bank bases its actions on the present value of its expected marginal tax rate through time. Since managers decide which subsidiaries should realize securities gains and losses before year's end, they make these decisions before the bank's exact state tax rate for the year is known.

We also examine the sensitivity of our results to the use of reported state tax expense by substituting total tax expense, which includes federal, state, local, and foreign tax expense. Given the lack of variation in the federal tax rate, variation in total taxes should be driven by state and local tax differences.[13] The correlation between total-tax-based effective state tax rates and our original measure is 71 percent and the signs and significance levels of the results reported in Table 5 are substantially unchanged when we use this alternative measure.

Sensitivity to Time Period Examined

Regulatory changes and data availability limit the time period available to study the effects of taxation on banks' interstate income shifting. Since the investment securities banks hold are almost exclusively debt instruments and the majority of the securities held are issued by the U.S. Department of the Treasury or other agencies, changes in the values of these securities are driven almost entirely by changes in interest rates. We perform several sensitivity checks to ensure that our results are not merely an artifact of the interest rate environment in the 1991–1992 period.

Our primary analysis excludes years before 1991 because the amount of interstate banking activity was substantially lower and because a change in capital regulation occurred in the last quarter of 1990. Although we cannot construct consistent capital measures before 1991, we can use equity as a noisy measure of banks' capital management incentives.

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[13] The median bank in our sample is unlikely to have any foreign operations and therefore will not pay foreign taxes.

Substituting an equity-to-assets ratio for the regulatory capital ratio, and expanding the sample period from 1991–1992 to 1988–1992, we re-estimate Equations (1)–(3). We find results similar to those reported in Table 5 except that banks' realized security gains and losses are sensitive to their own tax rates only in Equations (2) and (3) when we control for the tax rates of other group members. Since this period includes years with both increasing and decreasing interest rates and with both steepening and flattening yield curves, this sensitivity test provides some assurance that our results are not driven by the economic environment in 1991 and 1992.

To examine the possibility that banks in different states may select systematically different portfolios of securities, we perform two additional tests on both our primary sample and our extended sample, which includes data from 1988–1992. With these tests we examine whether our results are mechanically driven by changes in total security gains and losses accruing during our sample period, rather than by banks' voluntarily realized gains and losses, as we hypothesize. First we re-estimate models (1)–(3) substituting total security gains and losses for realized security gains as the dependent variable. As expected, if our results are not attributable to the economic environment and banks' self-selection biases, we find no evidence of either tax management or earnings management in models where total security gains is the dependent variable.

We further examine whether our tax results are due to fluctuations in total security gains by re-estimating models (1)–(3), using as an alternative dependent variable the ratio of realized security gains to total security gains. If the variation in realized security gains is driven by fluctuations in total security gains, then the variation in the denominator and numerator would cancel out and we would not expect to see tax results similar to those in our primary tests. In contrast, if the variation in realized security gains is due to banks' strategic choices, then we would continue to find results similar to those reported for the primary analyses.

The inferences from estimating our models using this alternative dependent variable are similar to those reported in Table 5. Banks are sensitive to the effective state tax rates of the other members of their consolidated groups, measured using either the average rate for those members or the group's highest and lowest state tax rates. We also find that banks are sensitive to their own tax rates, although, again, we find this only when we also control for the tax rates of other group members in Equations (2) and (3).

The consistent results from sensitivity tests suggest that it is unlikely that our reported results are simply attributable to economic conditions.

Sensitivity to Measurement of Consolidated Group Earnings

We also assess the sensitivity of our results to our measurement of pre-securities-gain earnings, capital, and unrealized gains for the other banks in the groups. The regression reported in Table 5 includes the mean of the ratio of these variables to total assets for the other banks in the group. Substituting the ratios computed for the consolidated group produces similar results.[14]

Robustness of Results to Multicollinearity

We perform a Belsley, Kuh, Welsh analysis for our three regression equations to allay concerns that collinearity is a factor in our findings. We find one condition index exceeding

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[14] We compute the ratios for the consolidated group by (1) summing the numerator for all other banks in the group, (2) summing the denominator for all banks in the group, and then (3) calculating the ratio (rather than calculating a ratio for each bank and then averaging those ratios).

30 in each of Equations (1) and (2). However, the proportion of the variance of each of our variables of interest is well below the recommended cut-off of 50 percent. Therefore, collinearity does not appear to affect our inferences in these two equations. In Equation (3) we find two condition indexes that exceed 30, and the proportion of the variance on our $\text{TaxRate}_{\text{High}}$ variable exceeds the recommended level. Therefore the insignificance of this variable may be attributable to an increased standard error due to collinearity.[15] The results of our regressions provide further evidence that our analyses and inferences are relatively free of multicollinearity difficulties. Specifically, we do not observe large changes in estimated coefficients when we conduct our sensitivity tests for alternative samples and specifications. Also, we do not observe large increases in the standard errors when we add the within-group tax measures in models (2) and (3), which are the variables most highly correlated with the bank's own state tax rate.

Robustness of Results to Influential Observations

We also perform a Belsley, Kuh, Welsh analysis for our three regression equations to examine the sensitivity of our findings to influential observations. Eliminating the nine observations this analysis identified as potentially influential does not change any of our inferences, and strengthens the significance of most of our variables of interest. In addition, we do not observe large changes in the coefficients on these variables after removing these observations. This evidence suggests that our results are not driven by a few influential observations.

VIII. CONCLUDING COMMENTS

In this paper we examine whether banks owned by interstate multibank holding companies coordinate their security gains and losses to manage their tax, earnings, and capital management objectives. Specifically, we examine whether the realization of security gains and losses is related to the objectives of the individual bank, or the consolidated group, or both.

Consistent with our tax management hypothesis, we find that banks manage their gain realizations to reduce their own state taxes. Additionally, we find that banks behave strategically to reduce their consolidated groups' tax expense. Finally, we find that measuring a bank's tax rate relative to the tax rates for other group members provides a more powerful design for detecting cross-jurisdictional tax planning than only considering a subsidiary's own rate, as in previous research.

Our findings are consistent with multistate affiliations adversely affecting the ability of high-tax states to collect taxes from banks. Thus, our evidence is inconsistent with Congressional testimony suggesting that banks do not engage in such cross-state tax shifting. This is especially interesting given that, during the period we study, multistate banking was conducted via legally independent, separately incorporated subsidiaries and not as branches where there are fewer restrictions on tax shifting. If we could obtain data to examine bank holding companies' abilities to shift income among states after the implementation of interstate branching laws, we would expect the results to be even more striking. Unfortunately, regulators do not require banks to disclose branch-level data.

We also capitalize on the within-group design to study how consolidated banking groups assign realizations of security gains and losses among their subsidiaries to manage their publicly reported income statements. Consistent with our financial statement earnings management hypothesis, we find banks realize security gain and loss to manage not only their own earnings, but also their group's publicly reported, consolidated earnings. Our finding that subsidiaries strategically recognize gains and losses to benefit the consolidated group is consistent with Beatty and Harris' (1995) conclusion that bank holding companies use earnings management to reduce information asymmetry between managers and shareholders.

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[15] This is supported by the fact that for our total sample the proportion of variance in our $\text{Tax Rate}_{\text{High}}$ variable drops below 50 percent and $\text{Tax Rate}_{\text{High}}$ is significant for the total sample.

We find no support for our capital management hypothesis at either the bank or group level. These results are consistent with Beatty et al. (1995) and Beatty and Harris (1999), and suggest that the lack of evidence of capital management in those studies is not attributable to their focus on holding-company-level data.

We perform a number of sensitivity analyses to confirm that our results are not sensitive to the banks included in our sample or to the time period studied. We find that our inferences are unchanged both when we include banks in states that do not tax income from U.S. obligations and when we include banks that are privately held. We provide evidence that our results are not driven by the economic condition during our sample period by extending our sample period and by demonstrating that our results do not arise from a spurious relation with total available gains.

We also examine the sensitivity of our results to our measure of the marginal state tax rate, which is the median effective tax rate of banks operating in the state. We find that, although our results are insensitive to allowing the coefficients on our median effective tax-rate measures to vary for banks with nonpositive income, our results are not robust to using bank-specific effective tax rates. We argue that our measure reduces measurement error contained in bank-specific effective tax rates, which evidence suggests is substantial for our sample. We provide evidence of implausible variability in the estimated bank-specific tax rates, lending credence to claims made in previous research that such estimates are so noisy that they are of limited value.

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Honesty in Managerial Reporting

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ABSTRACT: This study reports the results of three experiments that examine how preferences for wealth and honesty affect managerial reporting. We find that subjects often sacrifice wealth to make honest or partially honest reports, and they generally do not lie more as the payoff to lying increases. We also find less honesty under a contract that provides a smaller share of the total surplus to the manager than under one that provides a larger share, suggesting that the extent of honesty may depend on how the surplus is divided between the manager and the firm. The optimal agency contract yields more firm profit than a contract that relies exclusively on honest reporting. However, a modified version of the optimal agency contract, which makes use of subjects' preferences for honest reporting, yields the highest firm profit. These results suggest that firms may be able to design more profitable employment contracts than those identified by conventional economic analysis.

Keywords: *experiment; honesty; incentive contract; managerial reporting.*

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I. INTRODUCTION

Managerial reporting communicates vital information within firms. Managers with local information report to upper management to inform production, marketing, and capital budgeting decisions. While communication can potentially increase firm profit and the manager's welfare, agency analyses of communication (e.g., Baiman and Evans 1983; Penno 1984; Melumad and Reichelstein 1987) assume that managers' opportunistic reporting limits the value of communication.

This study examines whether managers' preferences include honesty as well as wealth, and whether managers' preferences for honesty increase the value of communication. To the extent that managers prefer to report honestly, firms may not need to limit the contracts they offer to those in which reporting truthfully maximizes the manager's wealth. Baiman and Lewis (1989, 2) examined whether preferences for honesty could increase the value of communication, noting that "many subtle but potentially important characteristics of communication are ignored in the agency model." However, they interpreted their experimental results as supporting the agency theory assumption that preferences for honesty are weak and therefore can be ignored in contracts designed to maximize firm profit.

We conduct a series of experiments to reconsider the importance of preferences for honesty. Our experiments have four objectives: (1) to reassess the empirical importance of preferences for honesty, (2) to provide evidence regarding the strengths and weaknesses of existing models of managerial reporting (i.e., communication), (3) to investigate factors that affect the extent of honest reporting, and (4) to provide evidence concerning how the extent of honesty affects firm profit.

With respect to the first objective, our findings reveal considerably more honest reporting than the conventional agency model suggests. This greater level of honesty is inconsistent with Baiman and Lewis' (1989) conclusion that preferences for honesty are too small to be important. Regarding our second objective, existing reporting models cannot explain important aspects of the reporting behavior we observe. Neither conventional agency models nor types models (Koford and Penno 1992) can explain reports that are "partially honest," i.e., reports that are neither purely wealth-maximizing nor purely honest. "Trade-off models," in which individuals trade off utility for wealth and disutility for lying (Brickley et al. 1997; Luft 1997) can potentially explain the existence of partially honest reports. To date, however, such trade-off models offer only limited guidance concerning the extent of partially honest reports.

Our third objective is to investigate factors that influence the extent of honesty. Brickley et al.'s (1997) trade-off model predicts that the level of honesty will decline as the payoff to lying increases. Our experimental results reveal little evidence of such a relation, suggesting instead that reporting may depend on how the contract divides the total surplus between the manager and the firm.

Our fourth objective is to assess how reporting behavior affects firm profit. We compare firm profit under alternative contracts that employ a reporting feature (i.e., reliance on honest reporting) and/or a production feature (i.e., dependence of the level of production on the manager's report). Our results confirm the agency theory prediction that a contract with the production feature yields more firm profit than a contract with only the reporting feature. However, as we demonstrate later in the paper, the difference in profit between the two contracts is smaller than the difference agency theory predicts. Further, while agency theory assumes that there is no value to adding the reporting feature to a contract that already incorporates the production feature, we demonstrate that firm profit increases when the contract employs both features. This finding suggests that the reporting feature, which relies exclusively on managers' preferences for honesty, can increase firm profit.

Finally, our results suggest that further research on individual reporting has considerable potential to improve contract design. The conventional agency model of reporting is simple, tractable, and captures the important influence of self-interest. However, our results suggest that honesty also plays an important role in reporting behavior. Overall, our results suggest an expanded view of managerial reporting that encompasses the effect of honesty on firm profit and recognizes that the extent of honesty may depend on how a contract divides the total surplus between the manager and the firm. Therefore, firms may be able to design employment contracts that are more profitable than those suggested by conventional economic analyses.

II. BACKGROUND

Virtually all ethical systems encourage honesty (Murphy 1993), in part to increase efficiencies and thus the total surplus (Arrow 1974; Noreen 1988). Kohlberg's (1969, 1981) theory of moral reasoning addresses how individuals internalize ethical behavior and suggests why individuals may prefer honesty. Although the economics literature has recognized individuals' preferences for factors in addition to wealth (e.g., Becker 1993; Jensen and Meckling 1994), formal economic models have only recently begun to include such preferences (e.g., Bolton 1991; Rabin 1993; Fehr and Schmidt 1999; Bolton and Ockenfels 2000).

This study investigates the role of honesty in a reporting setting in which local managers have superior information that headquarters needs to make efficient operating decisions. Organizations typically address such problems via their budgeting system. Previous experimental studies of such settings include Young (1985), Waller (1988), Chow et al. (1988), Waller and Bishop (1990), Chow et al. (1991), and Chow et al. (1994, 1995). While these studies have made important contributions to the budgeting literature, Waller (1994) and Young and Lewis (1995) suggest that further progress requires testing established economic theories and developing new theories that integrate behavioral and economic factors (see also Kachelmeier 1994, 1996; Luft 1997; Moser 1998). The current study adopts this approach.

Baiman and Lewis (1989) examine whether subjects experience disutility from lying. They interpret their results as indicating that each individual has a personal honesty threshold. Up to the threshold, the individual will report honestly; but once the threshold is reached, the individual will report to maximize his wealth. Baiman and Lewis (1989) conclude that this threshold is typically quite low, i.e., most individuals will lie for a payoff as small as $0.25. Consequently, Baiman and Lewis (1989) conclude that the assumed wealth-maximizing behavior in models of managerial reporting is appropriate.

Antle and Eppen (1985) and Antle and Fellingham (1995) examine how to elicit and use managers' private information most efficiently when the firm can adjust the level of production based on the manager's report. They employ the standard economic assumption that managers report to maximize their utility for wealth. The contract they identify as maximizing firm profit includes a production hurdle. It calls for production when the reported cost is low (at or below the hurdle) and no production when the reported cost is high (above the hurdle). The firm uses the manager's report only to determine whether there is production, but not as a measure of the resources to be transferred to the manager when there is production.

III. EXPERIMENTS

We conduct three experiments designed to examine managerial reporting and firm profitability in a budgeting setting. Subjects act as managers who privately observe the cost of production. Based on the manager's cost report, the firm provides the manager with resources. Managers can obtain excess resources by lying (overstating the actual production cost). Managers know that if they lie, their lies cannot be detected because the experimental setting precludes monitoring, auditing, or reputation effects.

Experiment 1: Trust Contract (TC)

Overview

Experiment 1 examines a setting in which the manager faces a direct trade-off between lying to maximize wealth and reporting honestly. That is, the more the manager lies, the more he earns, and the less he lies, the less he earns. Thus, the experiment provides a direct test of the relation between monetary incentives to lie and reporting behavior. Because the manager knows the actual cost for certain when making his cost report, risk aversion plays no role in the reporting decision. Likewise, there are no intervening complications such as how the manager's skill or productive effort affects production or the total surplus. Avoiding such complications is consistent with calls for simpler and sharper empirical tests comparing predictions from the conventional economic model to alternative hypotheses (Young and Lewis 1995; Luft 1997).

We refer to the contract used in Experiment 1 as the Trust Contract (TC) because the firm transfers to the manager whatever amount he requests. Because any payoff the manager claims reduces the firm's profit, the manager's report directly determines how the available payoff is divided between the manager and the firm.

Procedures

Twenty-eight M.B.A. students each assumed the role of a division manager who reports his private production cost to corporate headquarters in the form of a budget request. When the division manager was hired, both the manager and corporate headquarters knew the probability distribution of the division's uncertain costs. After beginning work on the job, but before submitting his budget request for that period, the manager's private forecasting system reliably determined exactly what the actual production costs would be for the coming period. Corporate headquarters, in contrast, knew only the probability distribution of possible production costs.

Managers submitted a budget request and kept any difference between the resources received from corporate headquarters and the actual production cost. In addition to keeping the excess of the budget request over the actual cost, subjects also received a salary of 250 Lira per period (30 Lira = \$1.00). Corporate headquarters never learned whether the manager's budget request was equal to the actual production cost (i.e., whether the manager reported his actual production cost honestly).

The firm sold 1,000 units of output for 6.00 Lira per unit. Actual production costs fell within the range of 4.00 Lira to 6.00 Lira per unit, and were randomly drawn from a set of 41 equally likely costs (4.00, 4.05, 4.10,...,6.00). The instructions provided several examples of how various budget requests would affect the manager's personal earnings and the division's contribution to firm profit. In addition, subjects received a comprehensive earnings spreadsheet that showed their personal earnings and their division's contribution to firm profit for all possible combinations of actual costs and budget requests.

The experiment consisted of ten independent periods. At the start of each period, each subject privately learned his or her actual cost per unit for that period. To expedite the experiment, we randomly determined the actual costs in advance. We randomly drew ten actual costs per unit (with replacement) from the set of possible costs (4.00, 4.05, 4.10,...,6.00). We repeated this procedure 28 times, generating 28 independent sets of ten actual costs per unit (one set for each of the 28 subjects).

After we read the instructions aloud and answered all questions, each subject randomly selected an envelope containing his or her confidential participant number and a set of ten budget request forms sequentially ordered from period 1 to 10. Each budget request form provided the randomly determined actual cost per unit for the current period. In addition, each form provided a table showing the subject's personal earnings and his/her division's contribution to firm profit for each possible budget request the subject could make. Subjects recorded on the form their budget request for the current period, their corresponding personal earnings, and their division's contribution to firm profit. Subjects were not permitted to look ahead to subsequent periods, and we collected all budget request forms for the current period before subjects could proceed to the next period.

Subjects' cash payments were based on one period selected randomly at the end of the experiment. While subjects completed a post-experimental questionnaire, an independent assistant located in a separate payment room converted subjects' earnings for the selected period to dollars at the rate of 30 Lira = \$1.00. Subjects used their confidential participant numbers to claim their cash payments in the payment room. The assistant was unaware of the purpose of the experiment, and the budget request forms for the randomly determined payment period identified subjects by their confidential participant numbers only. These procedures, along with subjects' anonymous participation, were designed to make clear that the experimenters would never know whether any individual subject lied or reported honestly. In this way, the experiment gave the conventional agency model, which predicts that all managers will report opportunistically, its best shot.

Results

Post-experimental questionnaire. Subjects' post-experimental questionnaire responses indicated that they understood the experimental task and procedures. All subjects believed their responses were anonymous and 96 percent understood that the firm would never know whether their reports were honest. Furthermore, 96 percent understood that their earnings for any period were not affected by their earnings in any other period or their division's contribution to firm profits in any other period.

Measuring honesty. We measure the percentage of honesty as $\pi = 1 -$ payoff claimed/payoff available, where the "payoff claimed" is the amount of payoff the subject earns based on the subject's actual report, and the "payoff available" is the amount that a subject could earn by lying to the maximum extent. For example, for an actual cost draw of 5.00 Lira per unit, the "payoff available" to the manager was 1,000 Lira. That is, by reporting 6.00 Lira per unit the manager would receive 6,000 Lira from the firm to produce 1,000 units, of which 5,000 Lira would be used for production, yielding a payoff available of 1,000 Lira. If instead the subject reported 5.60 Lira per unit, the firm would transfer 5,600 Lira to the manager. The manager would use 5,000 Lira for production, yielding a "payoff claimed" of $5{,}600 - 5{,}000 = 600$ Lira. The percentage of honesty (π) would equal 1 − payoff claimed/payoff available = $1 - 600/1{,}000 = 40\%$. If the subject reported 5.00 Lira per unit (i.e., reported completely honestly), then π would equal 100%; and if the subject reported 6.00 Lira per unit (reported to maximize wealth), π would equal 0%.[1] Therefore, π measures the extent of honesty as the percentage of the payoff available that subjects *did not* claim.

A relatively small number of reports (18 of 280 total reports in Experiment 1) were for costs *less* than the actual cost. Such reports are inconsistent with subjects' trading off wealth and honesty because they would have received a higher payoff by reporting honestly. All analyses in this paper are based on data adjusted by replacing such reports with the actual cost. The rationale for this adjustment is that our study focuses on the trade-off between wealth and honesty, and reports of costs that are less than actual costs are inconsistent with any combination of preferences for wealth and honesty. Because the adjustment results in a *lower* percentage of honesty (π) and *lower* firm profit, it ensures that the amount of honesty included in our analysis is not inflated by cost reports that were less than the actual cost. Moreover, our statistical conclusions are unaffected by the adjustment except in one case, and for that case we report results for both the adjusted and unadjusted data and explain why the results differ.

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[1] The Appendix explains how we extend the calculation of π to multiple reports by multiple subjects.

Conventional agency model. The conventional agency model assumes that individuals experience no disutility from lying. Thus, the agency model predicts subjects will always claim the entire payoff available; and hence π is expected to be 0% for all reports. In contrast to this prediction, Panel A of Table 1 shows that the percentage of honesty (π) in Experiment 1 is 48.7%, and that the mean and maximum payoffs that subjects gave up by making honest or partially honest reports were \$16.07 and \$61.67, respectively.[2] The high percentage of honesty and the large dollar payoffs subjects gave up are clearly inconsistent with the conventional agency model's prediction of wealth-maximizing reporting.[3]

Moreover, if subjects had reported to maximize their wealth, then their reports should be unrelated to their actual production costs because they would have always reported 6.00 Lira. To test this prediction, we regressed subjects' reported costs on their actual costs. Contrary to the prediction of no relation, we find a significant positive association ($\beta = +0.450$, $p < 0.001$) between reports and actual costs.[4] This finding is consistent with subjects having a preference for honesty, but inconsistent with wealth-maximizing reporting or with random reporting, both of which imply no correlation between actual and reported costs.

Additional evidence that preferences for honesty affected subjects' reports comes from a comparison of the results of Experiment 1 with the experimental results for dictator games. A subject's reporting decision in Experiment 1 is economically equivalent to a subject's decision in a dictator game, in which the subject (dictator) decides how to divide a fixed payoff with another party. In dictator games with settings like Experiment 1, in which the dictator never meets or even sees the other party, the dictator gives, on average, about 18% of the total amount available to the other party and keeps about 82% for himself (Bohnet and Frey 1999; Bolton et al. 1998; Eckel and Grossman 1996; Forsythe et al. 1994; Hoffman et al. 1994).[5] Despite its economic equivalence to a dictator game, Experiment 1 is behaviorally quite different because subjects must lie to get more of the payoff, a feature not present in dictator games. Thus, subjects' preferences for honesty apparently cause them to give more of the available payoff to the other party in Experiment 1 (48.7%) than in anonymous dictator games (18%).

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[2] Possible payoffs range from \$0 to \$66.67, where the \$66.67 payoff results from an actual cost of 4.00 Lira per unit, a reported cost of 6.00 Lira per unit, production of 1,000 units and a conversion rate of \$1 per 30 Lira.

[3] Mean reported costs are approximately the same across the ten experimental periods for this and all subsequent experiments, indicating that subjects did not lie more as they gained experience with the task.

[4] Results are for the regression model: Reported Cost = $\alpha + \beta$ Actual Cost, with the error term adjusted for repeated measures across the 28 subjects. The analysis included all observations except six cases with an actual cost of 6.00 in which there is no payoff available from lying, producing a sample of $n = 280 - 6 = 274$. Because the range of the dependent variable is bounded at 4.00 and 6.00, we repeated the analysis using two-sided censored regressions (tobits), and the results are substantively the same ($\beta = +0.505$, $p < 0.001$).

[5] In "anonymous" dictator games, the dictator never meets nor sees the other player. The percentage (18%) is an unweighted average of the results of anonymous dictator games reported in the five studies cited in the text. Mean percentages given to the other player in these studies ranged from 9.6% to 26%.

TABLE 1
Percentage of Honesty, Payoff Forgone and Report Types under the Trust, High Payoff Trust, and Modified Trust Contracts

Panel A: All Observations

	Experiment 1	*Experiment 2*	*Experiment 3*
	Trust Contract (TC)	*High-Payoff Trust Contract (HPTC)*	*Modified Trust Contract (MTC)*
	n = 280	*n = 110*	*n = 280*
Percentage of Honesty[a] (π)	48.7%	42.9%	NA
Payoff Forgone[b]			
Mean ($)	$16.07	$ 68.63	NA
Maximum ($)	$61.67	$291.67	NA
Report Types[c]			
Economic ($\pi = 0\%$)	27%	29%	NA
Honest ($\pi = 100\%$)	25%	21%	NA
Other ($0\% < \pi < 100\%$)	48%	50%	NA

Panel B: Actual Cost ≤ 5.00

	n = 142	*n = 55*	*n = 142*
Percentage of Honesty[a] (π)	47.7%	41.0%	21.8%
Payoff Forgone[b]			
Mean	$23.81	$100.00	$ 3.61
Maximum	$61.67	$291.67	$31.67
Report Types[c]			
Economic ($\pi = 0\%$)	22%	18%	61%
Honest ($\pi = 100\%$)	16%	9%	16%
Other ($0\% < \pi < 100\%$)	62%	73%	23%

Panel C: Actual Cost > 5.00

	n = 138	*n = 55*	*n = 138*
Percentage of Honesty[a] (π)	52.0%	49.1%	NA
Payoff Forgone[b]			
Mean	$ 8.12	$ 37.27	NA
Maximum	$31.67	$150.00	NA
Report Types[c]			
Economic ($\pi = 0\%$)	33%	40%	NA
Honest ($\pi = 100\%$)	33%	33%	NA
Other ($0\% < \pi < 100\%$)	34%	27%	NA

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[a] The percentage of honesty (π) is calculated as $1 - (\Sigma_{i=1}^{n} \text{Payoff Claimed}_i / \Sigma_{i=1}^{n} \text{Payoff Available}_i)$. That is, π is 1 minus the ratio of payoff claimed for all observations divided by payoff available. However, in Experiment 3 when the actual cost > 5.00, the payoff available is zero, and hence π is not defined. Therefore, for Experiment 3 we report data for actual cost ≤ 5.00 only (Panel B) and show NA (Not Applicable) in Panels A and C because the payoff available is zero for all observations in Panel C and for some observations in Panel A.

[b] Payoff forgone is the amount of additional profit that the subject could have received by lying to the maximum (i.e., the difference between the payoff available and the payoff claimed).

[c] We categorize each report as economic ($\pi = 0\%$), honest ($\pi = 100\%$), or other ($0\% < \pi < 100\%$) based on the percentage of payoff available the report claimed. The percentage of each type of report is reported in the table.

Threshold model. Baiman and Lewis' (1989) threshold model assumes that because individuals experience a small fixed disutility from lying, they report honestly for all payoffs less than their personal disutility threshold and report to maximize wealth for all payoffs at or above the threshold. The last row of Panel A of Table 1 shows that in Experiment 1, 48 percent of subjects' reports were partially honest, i.e., these reports resulted in the subject's receiving some, but not all, of the payoff available from lying. That is, approximately one-half of the reports in Experiment 1 are inconsistent with the threshold model's prediction that subjects will either report honestly or report to maximize wealth.

Types model. The high percentage of partially honest reports in Experiment 1 is also inconsistent with Koford and Penno's (1992) types model, in which agents are one of two types: "ethical" (fully honest) or "economic" (willing to tell any lie necessary to maximize wealth). Ethical types never lie because they experience infinite disutility from lying, whereas economic types always lie to maximize their wealth because they experience no disutility from lying (as in the conventional agency model). In Experiment 1, 18 percent of the subjects always reported to maximize their wealth (i.e., $\pi = 0\%$ in every period), 7 percent always reported honestly (i.e., $\pi = 100\%$ in every period), and the remaining 75 percent made partially honest reports in some or all periods. Corresponding analyses for Experiments 2 and 3 show that the results for individual *subjects* (not reported) parallel the results for individual *reports* (see Table 1). Thus, for Experiments 2 and 3 we report results on an individual report basis only.

Experiment 2: High Payoff Trust Contract (HPTC)

A potential limitation of Experiment 1 is that the payoffs to lying ranged from $0.00 to $66.67, depending on the actual cost draw. While these payoffs are high relative to those used in most experimental economics studies, higher payoffs might lead to less honest reporting. Accordingly, we conducted Experiment 2, which was identical to Experiment 1 except that the dollar payoffs for lying were increased by a factor of 5. Thus, the maximum payoff for lying under Experiment 2 was $333.33 as opposed to the maximum of $66.67 under Experiment 1. We refer to the contract used in Experiment 2 as the High Payoff Trust Contract (HPTC). To control cost, we limited Experiment 2 to 11 subjects.

Results

As in Experiment 1, subjects' post-experimental questionnaire responses indicated that they understood the experimental task and procedures. All subjects understood that their responses were anonymous and that the firm would never learn whether they were honest. In addition, all subjects understood that their earnings for any period were not affected by either their earnings for any other period or their division's contribution to firm profit.

Panel A of Table 1 shows that the percentage of honesty (π) in Experiment 2 was 42.9%, and that the mean and maximum payoffs forgone by making honest or partially honest reports were $68.63 and $291.67, respectively (of a possible range from $0 to $333.33).[6] As in Experiment 1, these results are clearly inconsistent with the conventional agency model's prediction that there will be no honesty in reporting ($\pi = 0\%$). Also, as in Experiment 1, a regression of reported costs on actual costs reveals a significant positive relation ($\beta = +0.340$, $p < 0.03$), suggesting that it is subjects' preference for honesty that causes them to deviate from wealth-maximizing reporting. Finally, the percentage of partially honest reports in Experiment 2 (50 percent) is very similar to that in Experiment 1 (48 percent), demonstrating again that neither Baiman and Lewis' (1989) threshold model nor Koford and Penno's (1992) types model can fully explain subjects' reporting behavior.

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[6] In Experiment 2, one reported cost was less than the actual cost. The results reported in the paper include that report after it was replaced with the actual cost as in Experiment 1.

Trade-off model. While the conventional agency, threshold, and types models described above assume either no disutilty, a small fixed disutility, or infinite disutility from lying, a more general economic perspective (e.g., Brickley et al. 1997) suggests that individuals will trade off ethical behavior with the monetary payoff for unethical behavior. In our setting, this means individuals will lie more as the payoff to lying increases.

To test whether subjects lied more when the payoff to lying increased, we pooled the data from Experiment 2 (HPTC) and Experiment 1 (TC) because these two experiments were identical except that the dollar payoffs were five times greater in Experiment 2. Using the pooled data, we regressed the percentage of honesty (π) on three variables: the payoff available from lying (in Lira), a dummy variable for the HPTC, and the payoff available from lying interacted with the HPTC dummy.[7] Neither the HPTC dummy nor the interaction term was statistically significant ($p > 0.96$ and $p > 0.64$, respectively), indicating that the fivefold increase in payoffs did not produce more lying in Experiment 2 than in Experiment 1. Furthermore, the payoff available from lying had no significant effect ($p > 0.44$) on the percentage of honesty, which, together with the insignificant interaction term, indicates that subjects did not lie significantly more when the payoff to lying increased within either experiment. Thus, we find no evidence that subjects lied significantly more as the payoff to lying increased either across or within Experiments 1 and 2.[8]

Our first two experiments focus on reporting behavior. However, reporting behavior directly affects firm profit, and conventional economic analysis of managerial reporting settings derives contracts that maximize firm profit. Because in our setting such contracts include a production feature, our third experiment examines reporting behavior and firm profit under a contract that incorporates both reporting and production features.

Experiment 3: Modified Trust Contract (MTC)

Background

Antle and Eppen (1985) and Antle and Fellingham (1995) show that in settings like ours, the optimal contract from an agency-theory perspective includes a production hurdle, i.e., a cost above which there is no production. We refer to this contract as the Hurdle Contract (HC). Given our experimental parameters, the optimal HC sets the production hurdle at 5.00 Lira per unit, meaning that there is no production or transfer of resources to the manager when the manager reports a cost greater than 5.00 Lira per unit.[9] For any report less than or equal to 5.00 Lira per unit, the firm transfers 5000 Lira (5.00 Lira per unit × 1,000 units) to the manager for production, with the manager retaining any difference between the transfer and actual production costs.

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[7] We adjusted error terms for repeated measures across the 39 subjects. The analysis included all observations except nine cases with an actual cost of 6.00 in which there is no payoff available from lying, producing a sample of $n = 280 + 110 - 9 = 381$. The dependent variable is the percentage of honesty (π) for each observation. Because the range of the dependent variable is bounded at 0.0 and 1.0 when using the adjusted data, we also ran a two-sided censored regression (tobit), which yields the same conclusions as those reported in the paper.

[8] Repeating the regression analysis using the unadjusted data (including reports of less than actual costs) yields the same statistical inferences, except that the effect of payoff available from lying is marginally significant ($p = 0.09$). However, this result is *not* due to subject's lying more as the payoff to lying increased. Rather, it reflects the fact that almost all reports of costs less than actual costs occurred when the payoff to lying was low. That is, reports of costs less than the actual cost were not random, but instead almost always occurred when the distribution of the total surplus was very unfavorable to the firm. In these cases, subjects claimed none of the payoff available to them for lying, and even gave the firm some of the payoff they could have had by reporting honestly, so that $\pi > 100\%$. Consequently, our conclusions regarding the effect of payoff available on the extent of lying are the same whether the statistical analysis is based on the adjusted or unadjusted data.

The optimal production hurdle is 5.00 Lira per unit because this value just balances the benefit to the firm of limiting the maximum transfer to the agent against the loss to the firm from not producing. The firm benefits because the HC limits the maximum transfer to the manager to 5,000 Lira as compared to a maximum of 6,000 Lira under the TC. The firm loses because there is no production when the cost falls above the hurdle (5.00 Lira) even though such production would be profitable.

Design

Experiment 3 introduces the Modified Trust Contract (MTC), which is the Trust Contract from Experiment 1 with a production hurdle. That is, the MTC retains the reporting feature from the TC while adding the production hurdle (5.00 Lira) from the HC. The reporting feature under the MTC means that the manager receives resources directly proportional to his report for all reports ≤ 5.00 Lira. By comparing firm profit under the MTC (with both the reporting and production features) to firm profit under the TC (with only the reporting feature) and to firm profit under the HC (with only the production feature), we can assess the incremental effects on firm profit of the reporting and production features.[10]

Procedures

The MTC condition employed identical experimental procedures to those used in Experiments 1 and 2, with the exception of adding a production hurdle that specified for reports > 5.00 Lira per unit, there would be no production and the manager would receive only his salary of 250 Lira. For reports ≤ 5.00 Lira per unit, the transfer from the firm to the manager was the same as under the TC. That is, the firm transferred to the manager 1,000 times the reported cost per unit.

Twenty-eight new subjects in Experiment 3 (MTC) received the same 28 independent and randomly determined sets of ten actual costs that were used in Experiment 1. That is, subject #1 in Experiment 3 received the same set of ten randomly determined actual costs as subject #1 in Experiment 1, and so forth for each of the 28 subjects. Thus, the actual cost realizations were held constant across Experiment 1 (TC) and Experiment 3 (MTC). This ensures that any differences in firm profit across these contracts cannot be due to differences in cost draws across contracts.

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[9] Given our experimental parameters, the firm maximizes expected profit by choosing the hurdle cost per unit (H) to maximize:

$$\frac{(1{,}000H - 4{,}000)}{2{,}000} \times (6{,}000 - 1{,}0000H - 250) + \frac{(60{,}000 - 1{,}000H)}{2{,}000} \times (-250)$$

subject to the constraint that $4.00 \leq H \leq 6.00$. The fraction $(1{,}000H - 4{,}000)/2{,}000$ is the probability of a cost draw below H and $(6{,}000 - 1{,}000H - 250)$ is the corresponding firm profit. Likewise, $(6{,}000 - 1{,}000H)/2{,}000$ is the probability of a cost draw above H and (-250) is the corresponding firm profit. The solution is a hurdle cost of $H = 5.00$ Lira per unit.

[10] We do not provide experimental results for the Hurdle Contract (HC) because it does not require the manager to trade off reporting to maximize wealth vs. reporting honestly, which is the focus of our study. The truth-inducing feature of the HC means that by reporting honestly the manager maximizes his wealth. Thus, our analysis of firm profit compares expected profit under the TC and MTC (given the actual percentage of honesty) to expected profit under the HC assuming honest reporting.

Results

As in Experiments 1 and 2, subjects' post-experimental questionnaire responses indicated that they understood the experimental task and procedures. All subjects understood that their responses were anonymous, and 96 percent of the subjects understood that the firm did not know whether they were honest. In addition, 92 percent understood that their earnings for any period were not affected by their earnings for any other period, and 96 percent understood that their earnings were unaffected by their division's contribution to firm profit.

Reporting behavior. Reporting behavior in Experiment 3 is interesting only for cost draws $\leq$ 5.00, because for cost draws $>$ 5.00 managers do not face a trade-off between reporting to maximize wealth and reporting honestly. For cost draws $>$ 5.00, subjects maximize their wealth by reporting honestly. Therefore, for Experiment 3, we report data only for cost draws $\leq$ 5.00 (Table 1, Panel B) and base our analysis exclusively on these reports.

As in Experiments 1 and 2, subjects' reporting behavior under the MTC (π = 21.8%) is *inconsistent* with the conventional agency model prediction that π = 0%.[11] Furthermore, the mean and maximum payoffs that subjects gave up by making honest or partially honest reports are \$3.61 and \$31.67, respectively (of a possible range from \$0 to \$33.33). Also, as was the case for the first two experiments, there is a significant positive relation (β = +0.149, $p < 0.04$) between reported costs and actual costs, suggesting that preferences for honesty influenced subjects' reports. Finally, as in Experiments 1 and 2, the partially honest reports under the MTC (23 percent, from Panel B of Table 1) are inconsistent with Baiman and Lewis' (1989) threshold model and Koford and Penno's (1992) types model.[12]

Despite the similarities across experiments described above, a closer comparison reveals that the production hurdle under the MTC led subjects to report less honestly than they did under the TC or HPTC. Specifically, subjects reported less honestly under the MTC (π = 21.8%) than they did for the same cost draws (from Panel B of Table 1) under either the TC (π = 47.7%, F = 9.04, $p < 0.01$) or the HPTC (π = 41.0%, F = 6.25, $p < 0.02$).[13] To our knowledge, no existing reporting model predicts this increase in the extent of wealth-maximizing behavior in response to the hurdle feature. A potential explanation for the lower level of honesty under the MTC is that the hurdle feature provided subjects with less of the total surplus and the firm with more.[14]

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[11] The results reported in Panel B of Table 1 for actual costs $\leq$5.00 include six cases in which the subject reported a cost less than the actual cost. As in Experiments 1 and 2, we replaced these costs with the corresponding actual cost. For actual costs $>$5.00 in Panel C of Table 1, there were 34 reports of costs less than the actual cost. Although these reports do not affect our reported results, they are neither random errors nor do they appear to result from subjects misunderstanding the task. Rather, subjects appear to have knowingly sacrificed their own wealth to boost firm profit by ensuring that production would take place. All such reports were 5.00 Lira, the amount that minimized the cost to the subject (i.e., manager) of increasing firm profit by enabling production. Such reports occurred most often when the actual cost was only slightly above 5.00, again suggesting that subjects were most likely to report to increase firm profit when the cost to them was minimal.

[12] Although there are partially honest reports in all three experiments, the proportion of such reports (from Panel B of Table 1) under the MTC (23 percent) is smaller than under either the TC (62 percent, χ^2 = 43.6, $p < 0.01$) or the HPTC (73 percent, χ^2 = 41.6, $p < 0.01$). The proportion of such reports under the TC is not different from that under the HPTC (χ^2 = 2.0, $p > 0.20$).

[13] The statistics for the comparisons reported in the text, as well as for the additional comparisons reported below, are based on regressions like those used to analyze the pooled data discussed in conjunction with the results of Experiment 2. The comparisons could be affected by the fact that the payoff available from lying is smaller under the MTC than under the TC or HPTC because of the hurdle feature in the MTC (i.e., because of the hurdle, the payoff available under the MTC ranges from 0 to 1,000, as compared to a range of 0 to 2,000 under the TC or HPTC). Therefore, we also consider alternative comparisons that hold constant the range of the payoff available from lying at 0 to 1,000 by comparing results for cost draws $\leq$ 5.00 under the MTC to results under the TC and HPTC for cost draws $>$ 5.00. These comparisons again confirm that subjects reported less honestly under the MTC (π = 21.8% from Panel B of Table 1) than under the TC (π = 52.0% from Panel C of Table 1, F = 9.83, $p < 0.03$) or the HPTC (π = 49.1% from Panel C of Table 1, F = 4.76, $p < 0.04$).

To illustrate the difference between the distribution of the total surplus under the MTC (Experiment 3) vs. the TC (Experiments 1 and 2), assume that the actual cost is 4.00 Lira per unit and the percentage of honesty is $\pi = 50\%$. Under the TC, the manager would receive 1,250 Lira (transfer of 5,000 plus salary of 250 less 4,000 used for production), which is 62.5 percent of the total surplus. The firm would receive 750 (revenue of 6,000 less transfer of 5,000 and salary of 250), which is 37.5 percent of the total surplus. In contrast, given the same actual cost and percentage of honesty under the MTC, the manager would receive only 750 (transfer of 4,500 plus salary of 250 less 4,000 used for production), which is 37.5 percent of the total surplus, while the firm would receive 1,250 (revenue of 6,000 less transfer of 4,500 and salary of 250), which is 62.5 percent of the total surplus. This pattern, in which the manager's share of the total surplus for a given level of honesty is smaller under the MTC than under the TC, holds for all actual costs for which there is a payoff available for lying. Therefore, the lower percentage of honesty under the MTC ($\pi = 21.8\%$) vs. the TC ($\pi = 48.7\%$) and the HPTC ($\pi = 42.9\%$) appears to result from subjects lying more under the MTC to achieve a more favorable distribution of the total surplus.[15]

Further evidence that reporting under the MTC differed from that under the TC comes from a regression of the percentage of honesty of individual subject reports under the MTC (π) on the payoff available from lying. This analysis reveals a marginally significant reduction in the percentage of honest reporting ($\beta = -0.207$, $p = 0.065$) as the payoff to lying increases. As we found no such relation in the first two experiments, the unfavorable distribution that managers faced under the MTC appears to have led them to engage in a greater degree of wealth-maximizing reporting.

Figure 1 provides an additional perspective on how reporting varied across contracts by plotting the mean lie for each actual cost draw (4.00, 4.05,...,6.00) under the TC, HPTC, and MTC contracts in Panels A, B, and C, respectively. Each panel in Figure 1 shows the Economic Prediction, which is the mean lie the conventional agency model predicted for each cost draw. For example, for an actual cost draw of 5.00 in Panel A under the TC, the Economic Prediction is a lie of 1.00 because the conventional agency model predicts that the manager will report 6.00. The plot of Actual Lies in each panel shows the actual mean lie for each actual cost draw.

The three panels of Figure 1 reveal patterns of reporting that are consistent with our previous conclusions. First, on average, subjects reported so as to receive some, but not all, of the total available payoff. Second, consistent with less honesty under the MTC, the plot of Actual Lies falls closer to the Economic Prediction line under the MTC (Panel C) than under either the TC (Panel A) or the HPTC (Panel B). Next, we relate the observed reporting patterns under the different contracts to firm profit for these contracts.

Firm profit. Firm profit in our experiments depends on three factors: (1) the manager's contract; (2) how honestly the manager reports; and (3) the actual cost realizations. To focus on the first two factors, we eliminate the effect of actual cost realizations by replacing

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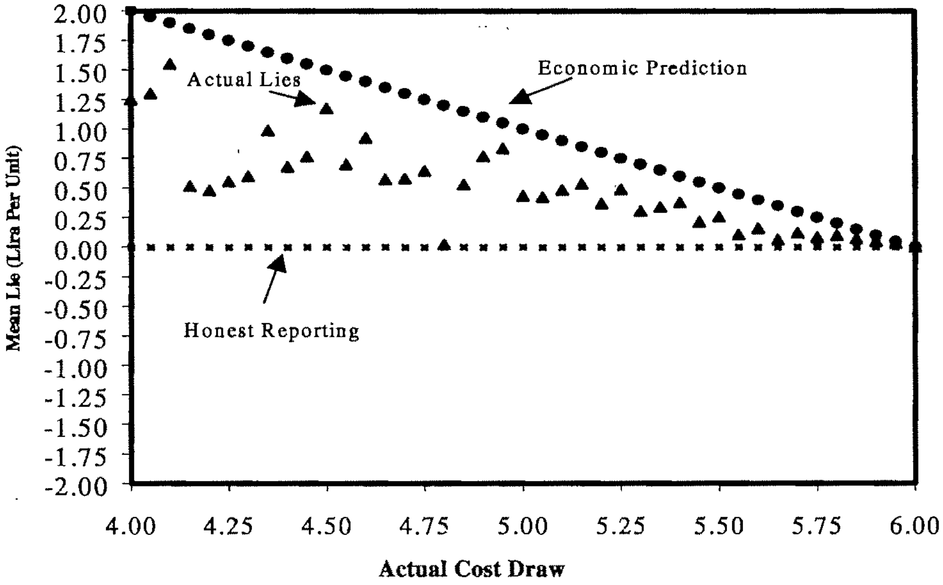
[14] We use "total surplus" to refer to the sum of the manager's and firm's total payoffs as distinguished from the "payoff available" which we use to describe the maximum payoff available to the manager for lying.

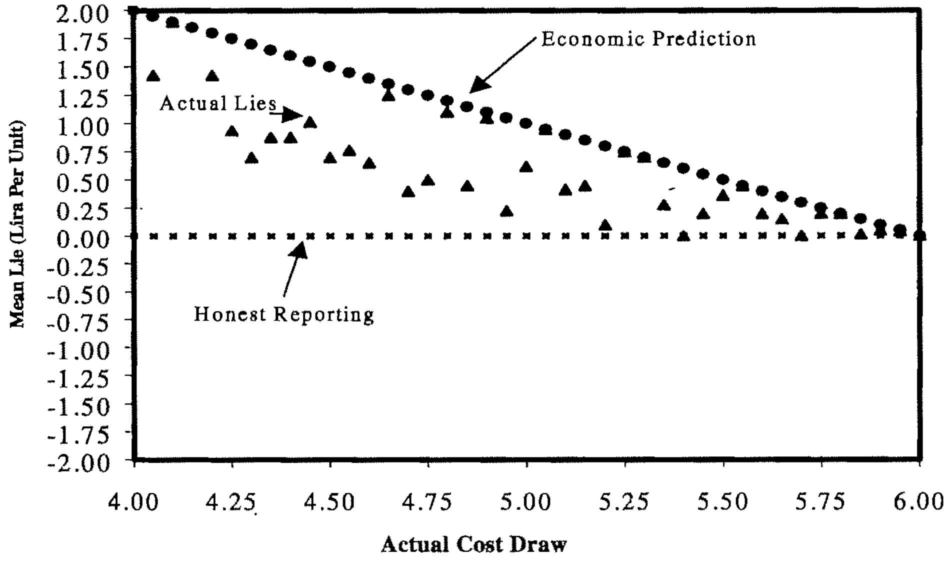
[15] Fehr and Gachter (2000) propose a similar argument for why a manager would reduce the level of his productive effort when faced with an incentive contract structured to deliver all or almost all of the available surplus to the principal and little or none to the manager.

FIGURE 1
Mean Lies by Actual Cost Draw for the Trust, High Payoff Trust, and Modified Trust Contracts[a]

Panel A: Mean Lie by Actual Cost Draw for Trust Contract (TC)



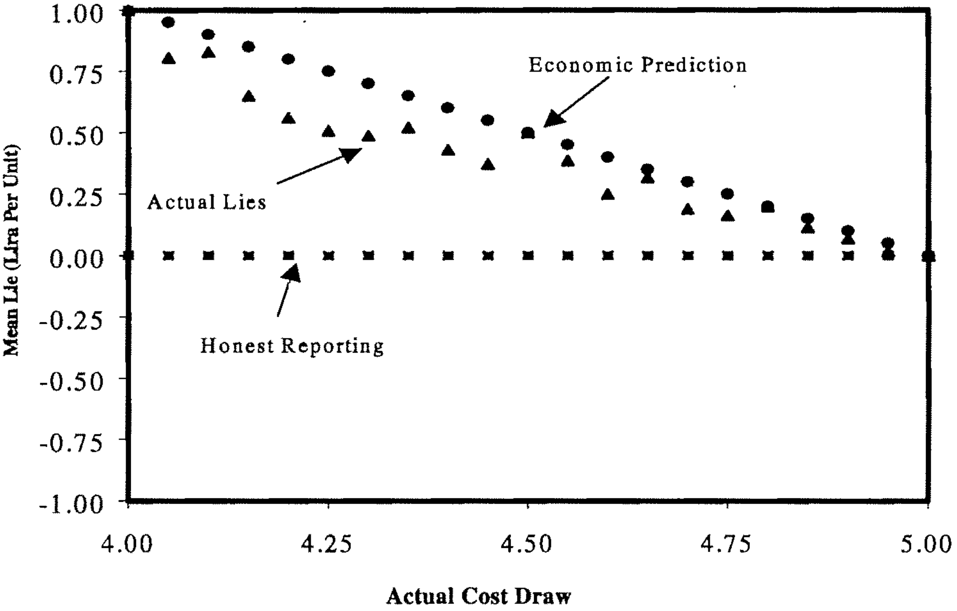
Panel B: Mean Lie by Actual Cost Draw for High-Payoff Trust Contract (HPTC)



(*Continued on next page*)

FIGURE 1 (Continued)

Panel C: Mean Lie by Actual Cost Draw for Modified Trust Contract (MTC)



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[a] "Mean Lie" is the mean value of (Reported Cost – Actual Cost), averaged over all cases of a specific cost draw. For example, under the TC, the actual cost draw was 5.00 in nine cases, and for those cases, the mean lie was 0.43 Lira.

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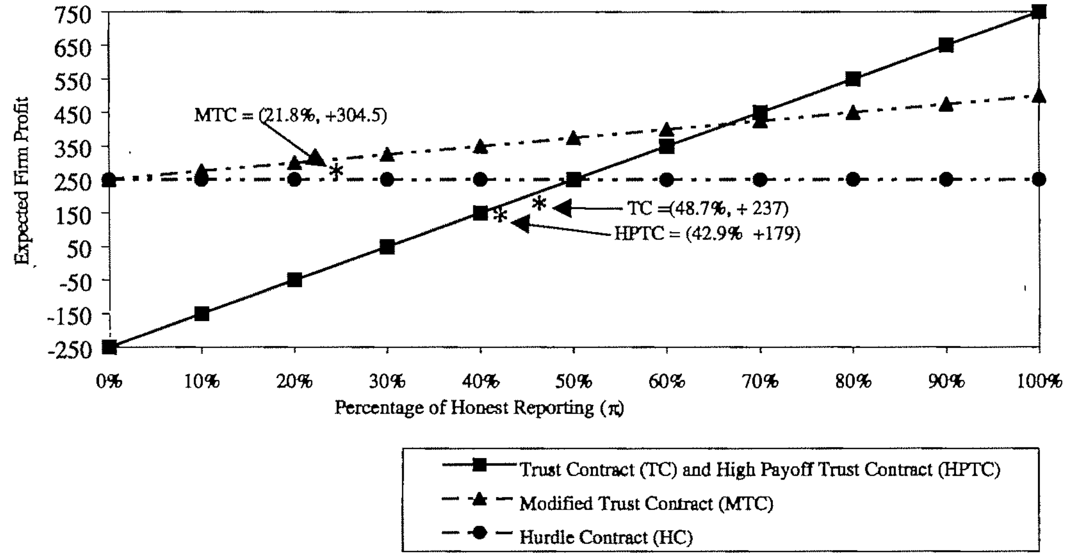
actual costs with the expected cost. Each contract's expected profit is then uniquely determined by the percentage of honest reporting (π) under that contract, as derived in the Appendix. That is, under the TC and HPTC, expected profit = $-250 + 1000\pi$, while under the MTC, expected profit = $250 + 250\pi$.

Figure 2 plots the resulting expected firm profit functions for each contract as the percentage of honesty (π) increases from 0% to 100%. The figure illustrates several important relations. First, for the standard economic assumption of wealth maximizing reporting ($\pi = 0\%$), the HC and MTC contracts both yield expected profit of +250 vs. an expected loss under the TC of –250. However, for any positive percentage of honesty ($\pi > 0\%$), expected profit under the MTC ($250 + 250\pi$) exceeds that under the HC (+250), illustrating that the MTC's reporting feature always increases firm profit for any positive percentage of honesty. Further, with sufficient honesty ($\pi > 50\%$), expected profit under the TC ($-250 + 1000\pi$) exceeds expected profit under the HC (+250), illustrating the potential for the reporting feature to produce more profit than the hurdle feature. Finally, for $\pi > 67.5\%$, expected profit under TC even exceeds expected profit under the MTC, illustrating that the reporting feature alone can generate more profit than a combination of the reporting and hurdle features if there is sufficient honesty.

The TC and MTC can both potentially yield greater firm profit than the HC. The TC can achieve greater profit than the HC in two ways: (1) producing a greater expected total surplus (1000, the first-best expected total surplus) than the HC (750, the second-best

FIGURE 2
Expected Firm Profit as a Function of the Percentage of Honest Reporting



* = Expected firm profit for actual percentage of honest reporting observed in experiments.

1. TC = (48.7%, +237) indicates that 48.7% is the actual percentage of honesty (π) and +237 is the expected firm profit per subject under the Trust Contract (TC). Expected firm profit is the value of the profit function for the TC from the Appendix, given the actual percentage of honesty as follows: −250 + 0.487 × (1,000) = +237.
2. HPTC = (42.9%, +179) indicates that 42.9% is the actual percentage of honesty (π) and +179 is the expected firm profit per subject under the High Payoff Trust Contract (HPTC). Expected firm profit is the value of the profit function for the HPTC from the Appendix, given the actual percentage of honesty as follows: −250 + 0.429 × (1,000) = +179.
3. MTC = (21.8%, +304.5) indicates that 21.8% is the percentage of honesty (π) and +304.5 is the expected firm profit per subject under the Modified Trust Contract (MTC). Expected firm profit is the value of the profit function for the MTC from the Appendix, given the actual percentage of honesty as follows: 250 + 0.218 × (250) = +304.5.

expected total surplus), and (2) distributing a greater percentage of this larger expected total surplus to the firm. In contrast, the expected total surplus under the MTC (750) is identical to that under the HC (750), and thus the only way the MTC can achieve greater firm profit than the HC is by distributing a greater percentage of this fixed total surplus (750) to the firm.[16] While a benefit of the TC is greater total surplus, a corresponding cost is greater exposure to opportunistic reporting by the manager. That is, the firm's expected profit can be as low as -250 under the TC if the manager reports to maximize his wealth ($\pi = 0\%$), while the hurdle feature of the MTC ensures that the firm's expected profit cannot fall below $+250$.

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[16] Because production always takes place under the TC, the expected total surplus of 1,000 under the TC is calculated as total revenue of 6,000 Lira less expected total cost of 5,000 (5.00 Lira per unit ×1,000 units). Because production takes place only when the actual cost $\leq$ 5.00 under the HC and the MTC, the expected total surplus of 750 under the HC and the MTC is calculated as 0.5(6,000 − 4,500) + 0.5(0) = 750. The first 0.5 is the probability of a cost draw less than or equal to 5.00, and the quantity (6,000 − 4,500) is 6,000 total revenue less the corresponding expected cost of 4,500. The second 0.5 is the probability of a cost draw greater than 5.00, and the quantity (0) is the corresponding total surplus when there is no production. Under both the TC and the MTC, the distribution of the total surplus between the manager and the firm depends on how honestly the manager reports.

We evaluate the expected profit relations depicted in Figure 2 using the actual percentage of honesty (π) observed under each contract. Figure 2 shows the actual percentage of honesty (π) and the corresponding expected firm profit for each contract as an asterisk "*".[17] We note several important findings. First, the comparison of expected firm profits across contracts given the conventional agency assumption of $\pi = 0\%$ does not apply because the actual percentage of honesty is well above zero for all contracts. Second, given the actual percentage of honesty of $\pi = 48.7\%$ under the TC, expected firm profit under the HC (+250) exceeds that under the TC (+237). However, this 13-Lira difference in expected firm profit is much smaller than the 500 Lira difference that agency theory predicts based on the assumption that $\pi = 0\%$. As a result, a relatively small increase in honesty under the TC would produce greater expected profit under the TC than under the HC. Firm policies that increase manager loyalty could potentially produce the increase in honesty necessary for firm profit under the TC to exceed that under the HC. Alternatively, other benefits associated with the TC might outweigh the small difference in firm profit. For example, the higher manager payoffs generated by the TC may result in increased worker effort or in lower worker turnover (Akerlof 1982, 1984; Shapiro and Stiglitz 1984; Stiglitz 1974).

Given the actual levels of honesty under each contract, expected firm profit with both the reporting and hurdle features under the MTC (+304.5) exceeds the expected profit with only the reporting feature under the TC (+237.0), as well as the expected profit with only the hurdle feature under the HC (+250.0). The MTC achieves greater expected profit despite having less honesty (21.8%) than under the TC (48.7%). While the MTC's hurdle feature results in less honesty than under the TC, it also reduces transfers to the manager, so that expected firm profit is greater by 67.5 Lira under the MTC than under the TC. The fact that the MTC's combination of reporting and a production hurdle yields greater expected firm profit than either the TC, which has reporting but no production hurdle, or the HC, which has a production hurdle but no reporting, demonstrates that both the reporting feature and hurdle feature are valuable. As noted earlier, by design, firm profit under the MTC will always be at least as great as under the HC. Nevertheless, our experimental results that $\pi = 21.8\%$ and firm profit is 304.5 are informative regarding the extent of the deviation from the agency predictions of $\pi = 0\%$ and firm profit of 250.0.

IV. DISCUSSION AND CONCLUSION

Our experiments reveal considerable honesty. In Experiments 1 and 2 subjects give up 48.7% and 42.9%, respectively, of the available payoff by reporting honestly or partially honestly. Of course, any level of honesty must be interpreted in light of subjects' reporting incentives. The Revelation Principle establishes that firms can always get honest reporting if they pay enough for it. From this perspective, the high level of honesty in Experiments 1 and 2 is particularly surprising because the firm paid nothing for it. That is, the contract was not designed to induce honest reporting. As a result, subjects had every reason to lie, because the more they lied the greater was their payoff, and because the absence of monitoring, auditing, and reputation effects meant that they faced none of the related negative consequences of lying.

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[17] We calculate expected firm profits conditional on the *actual* percentage of honesty (π) under each contract. Using expected profits eliminates any random influence of actual cost realizations. However, for completeness we note that the profit comparisons are unchanged when we use actual costs in place of expected costs. Under the TC, firm profit given actual cost draws is 232 vs. firm profit of 237 given expected cost draws. Likewise, under the HPTC, firm profit given actual cost draws is 162 vs. firm profit of 179 given expected cost draws. Finally, under the MTC, firm profit given actual cost draws is 312 vs. firm profit of 304.5 given expected cost draws.

To exploit managers' honesty most efficiently, firms must anticipate how alternative contract designs will affect reporting behavior and firm profit. Existing models cannot explain the reporting observed in our experiments. Conventional agency models assume there will be no honesty and Baiman and Lewis (1989) conclude that preferences for honesty are too weak to be important. Threshold and types models predict either 0% or 100% honesty for each subject, but we find that about half of the reports in Experiments 1 and 2 are partially honest.

The differences between our conclusions and those of Baiman and Lewis (1989) appear to reflect different experimental procedures and a different interpretation of their experimental results. With respect to experimental procedures, Baiman and Lewis (1989, 6) indicate that "through explicit instructions, examples, and computational worksheets we strongly encouraged subjects to use an expected value approach...." They justify this approach by explaining that "we guided the subjects to evaluate the *benefit* of each contract in terms of expected monetary value; but we provided no guidance for evaluating or even considering the cost of lying, which is the object of interest" (Baiman and Lewis 1989, 7). However, by instructing and training subjects to use expected monetary value maximization, the expected monetary benefit of each contract was made very salient, while the possibility that subjects might be reluctant to lie was never even mentioned. Yet, despite this strong encouragement to use an expected value approach, 41.7 percent of Baiman and Lewis' (1989, Figure 4, p. 13) subjects (20 of 48) still reported honestly, which is roughly comparable to the percentage of honesty we find for the Trust Contract (π = 48.7%) and High Payoff Trust Contract (π = 42.9%). Although their results provide some evidence that subjects lied more as the payoff to lying increased, 25 percent (4 of 16) of their subjects who could receive the highest payoff for lying (Baiman and Lewis 1989, Figure 4, Group 3, p. 13) still reported honestly. Thus, one can question whether their results are consistent with their assertion that "any advantage from exploiting an agent's reluctance to lie explicitly is likely, on average, to be small or nonexistent" (Baiman and Lewis 1989, 15).

Two alternative approaches that allow individuals to trade off utility for wealth and disutility for lying offer possible explanations for the high level of partially honest reports observed in this and earlier studies (e.g., Chow et al. 1988; Waller 1988). First, the trade-off approach proposed by Brickley et al. (1997) predicts that when managers' contracts increase the payoffs for unethical behavior, more unethical behavior will result. Our results provide only limited support for this prediction. The predicted negative relation between the extent of honesty and the payoff to lying is rejected in Experiments 1 and 2, and only weakly supported in Experiment 3.

Luft (1997) proposed a second trade-off approach that assumes individuals' disutility for lying increases in the size of the lie. That is, larger payoffs to lying increase both the utility from lying (via greater wealth) and the disutility from lying. Thus, individuals could maximize their utility by reporting to receive all, part, or none of the payoff available for lying, depending on the extent to which their utility for wealth and disutility for lying increase with the size of the lie. Although this approach is consistent with many of the existing empirical results, its generality limits its empirically testable implications.

Our results suggest a different approach to explaining the extent of honesty in managerial reporting, based on how the manager's report affects the distribution of the total surplus. This approach could potentially explain partially honest reports as well as our finding of less honesty under the Modified Trust Contract than under the Trust Contract. That is, it appears that the reason that individuals reported less honestly under the Modified Trust Contract is that they wanted to achieve a desired distribution of the total surplus between themselves and the firm. The potential ability to explain different levels of honesty across contracts represents an advantage of our distributional hypothesis over the approaches proposed by Brickley et al. (1997) and Luft (1997).

We expect our finding of less honesty under the Modified Trust Contract to generalize to other similar agency theory contracts that include both a reporting feature (i.e., the transfer to the manager is proportional to his report) and a production feature (i.e., the level of production depends on the manager's report). Because any production hurdle increases firm profit by reducing the payoff to the manager more than it reduces the total surplus, our distributional hypothesis predicts that managers would report less honestly under any contract with a production hurdle and the reporting feature than under a corresponding contract with only the reporting feature, such as the Trust Contract.

The distributional hypothesis we propose is consistent with recent theoretical developments in economics that consider factors other than wealth maximization, such as equity and reciprocity (e.g., Bolton and Ockenfels 2000; Fehr and Schmidt 1999; Rabin 1993; Bolton 1991). These recent models reflect efforts to reconcile empirical observations that are inconsistent with conventional economic models. To the extent that equity, reciprocity or distributional effects are important, conventional reporting models misrepresent the trade-off that firms make when they reduce production to increase firm profit. The conventional analysis assumes that reducing the manager's total payoff will not affect the honesty of the manager's report, but our results suggest that managers may report less honestly as their share is reduced.

If the distributional effects suggested by our results are confirmed in future studies, then firms may be able to increase the level of honest reporting by redesigning managerial compensation. For example, paying a larger fixed salary would increase the manager's share of the total surplus and reduce the firm's share, potentially prompting more honest reporting. Of course, the firm would trade off the benefits of more honest reporting against the additional cost of a larger fixed salary for the manager. Given the potentially important consequences of the distributional hypothesis for reporting behavior and firm profit, further research is necessary to clarify the extent to which distributional concerns affect reporting.

The discussion above suggests that firms may be able to design employment contracts with different trade-offs than those suggested by conventional economic analysis. Conventional agency analyses assume all individuals are willing to lie to maximize their wealth. In contrast, our results suggest that individuals' willingness to lie is endogenous, i.e., that the extent of honesty depends on the type of contract individuals receive. Therefore, the firm should select the optimal contract, taking into account the contract's effect on the manager's willingness to lie. A potentially important role of future research is to provide further empirical evidence concerning how contract design affects the honesty of reporting and firm profit.

Finally, the discussion above also suggests that firms may be able to make other adjustments to their control systems, broadly defined, to increase the honesty of reporting and firm profit. By adopting certain cost-effective human resource policies (e.g., profit-sharing arrangements), firms may be able to improve managers' attitudes toward the firm, and thereby increase the amount of honest reporting. Moreover, such policies may produce benefits beyond increasing the level of honesty (e.g., increasing worker effort or decreasing worker turnover). Such considerations may help explain the prevalence of human resource policies designed to increase worker loyalty. Given the importance of these issues and the central nature of reporting to accounting, our findings suggest the need for further research to clarify how factors other than conventional monetary incentives affect managers' reporting behavior.

APPENDIX

Expected Firm Profit

This appendix derives theoretical predictions for expected firm profit under the three contracts considered in the paper. Expected firm profit under each contract is expressed as a function of π, the percentage of honest reporting under that contract. Rather than analyzing total firm profit across all observations, we analyze firm profit per observation. The two analyses are equivalent because under all contracts subjects face the same distribution of expected costs.

For a given contract, expected firm profit depends on the expected cost per unit and on individual reporting behavior. The equations for expected firm profit developed below incorporate the expected cost per unit, based on the assumed uniform distribution of actual costs, and the mean percentage of honesty under that contract (π).

To measure π, we first define "payoff available" as the maximum amount that all subjects could have received across all periods if they lied to maximize their wealth. For example, under the TC and the HPTC, payoff available is based on all subjects reporting the maximum possible cost of 6.00 because that report generates the maximum possible payoff for the subjects. Next, we calculate the total amount of payoff actually claimed by all subjects across all periods, and refer to this as the "payoff claimed." Finally, we define π as 1 minus the ratio of payoff claimed divided by payoff available. Defined this way, π measures the percentage of honesty as the percentage of the payoff available that subjects gave to the firm by reporting honestly or partially honestly.

We use two subjects and a single period to illustrate the calculation of percentage of honest reporting (π), but the approach generalizes to more subjects and periods. Assume that under the TC, a subject gets a cost draw of 4.00 and reports 5.00, while a second subject gets a cost draw of 5.00 and reports 6.00. The payoff available is 3,000, determined as $1{,}000(6.00 - 4.00) = 2{,}000$ for the first subject and $1{,}000(6.00 - 5.00) = 1{,}000$ for the second subject. The payoff claimed would be 2,000, calculated as $1{,}000(5.00 - 4.00) = 1{,}000$ for the first subject plus $1{,}000(6.00 - 5.00) = 1{,}000$ for the second subject. Then the percentage of honesty for the two subjects would be $\pi = 1 - 2{,}000/3{,}000 = 33.3\%$.

The percentage of honesty calculated above is equivalent to the weighted average of the percentage of honesty for each report. We show this calculation because it provides further intuition regarding the economic meaning of π. The weights used in the weighted average are the total payoffs available to each subject. In the example, the first subject's percentage of honesty is $1 - 1{,}000/2{,}000 = 50\%$, and the weight is the payoff available of 2,000. The second subject's percentage of honesty is $1 - 1{,}000/1{,}000 = 0\%$, and the weight is the payoff available of 1,000. The weighted average is then $[50\%(2{,}000) + 0\%(1{,}000)]/3{,}000 = 33.3\%$. This example shows that, for these two cost draws, the first report is weighted twice as heavily because there is twice as much payoff available (2,000) as there is when the second report is made (1,000). For every cost draw the weights will vary across reports based on the payoff available.

Measuring honesty as we do above has two important advantages. First, it places the raw magnitude of any specific lie in perspective by relating it to the maximum possible lie for that cost draw. An alternative would be to measure honesty in terms of the size of the lies reported. However, using the example above, this would mean that a report of 5.00 when the actual cost was 4.00 would produce a lie of 1.00 Lira, and hence would not be distinguishable from the same size lie produced by a report of 6.00 when the actual cost was 5.00. We believe that the first report (5.00) is actually more honest than the second report (6.00) because the first subject received only one-half of the maximum that was available, while the second subject received the maximum that was available. Defining π as the percentage of honesty enables us to distinguish these two cases.

The second, and more important, advantage of defining π as the percentage described above is that doing so enables us to express firm profit under each contract as a function of π. These firm profit functions are essential both as background for interpreting the experimental results and also in evaluating the firm's trade-offs in contract design. We next explain the relation between π and firm profit, beginning with the Trust Contract.

Trust Contract

Under the TC, the firm's expected profit (EP) can be expressed as:

$$EP(TC) = E\{-250 + 1{,}000\pi(6.00 - AC)\},$$

where E indicates an expectation, AC is the actual production cost per unit, (6.00 − AC) is the actual profit margin per unit, and $\pi(6.00 - AC)$ is the firm's share of the profit margin per unit when the manager reports with a percentage of honesty of π. This expression demonstrates that reporting with honesty of π in this context is equivalent to "giving" the firm that percentage of the profit margin per unit. Substituting 5.00 for the expected cost per unit and simplifying the above expression yields:

$$EP(TC) = -250 + 1{,}000\pi.$$

Therefore, this analysis predicts that if all individuals were economic ($\pi = 0\%$), expected profit would be −250, if all individuals were honest ($\pi = 100\%$), expected profit would be +750, and if individuals were, on average, 50 percent honest ($\pi = 50\%$), expected profit would be +250.

Hurdle Contract

Under the HC, the firm's expected profit can be expressed as:

$$EP(HC) = pr(AC \leq 5.00)(6{,}000 - 5{,}000 - 250) + pr(AC > 5.00)(-250),$$

and using the fact that the probability of a cost draw of 5.00 or less is 0.5 under the assumed uniform distribution, the above expression becomes:

$$= .5(6{,}000 - 5{,}000 - 250) + .5(-250),$$

which reduces to +250.

The first term is expected profit when the actual cost is 5.00 Lira per unit or less, the manager reports 5.00 or less, production takes place, revenue is 6,000, and the transfer to the manager is the 5,000 fixed transfer plus his 250 salary. The second term reflects that when the actual cost is greater than 5.00 Lira per unit, the manager reports more than 5.00, there is no production, and the manager receives his salary of 250. The HC provides all subjects with an incentive to report honestly, *independent* of whether the individual has a preference for reporting honestly. As a result, all individuals will report honestly under the

HC, making firm profit independent of π. This is reflected in the fact that the expression, EP(HC) = +250 is independent of π.

Modified Trust Contract

Expected firm profit under the MTC is

$$\begin{aligned}&.5\{\pi[6{,}000 - 4{,}500 - 250] + (1 - \pi)[6{,}000 - 5{,}000 - 250]\} + .5\{-250\}\\&= .5\{\pi[500] + 750\} + .5\{-250\}\\&= 250 + 250\pi.\end{aligned}$$

The first term, ($.5\{\pi[6{,}000 - 4{,}500 - 250] + (1 - \pi)[6{,}000 - 5{,}000 - 250]\}$), is expected profit when the actual cost is 5.00 Lira or less. When the manager reports honestly ($\pi = 100\%$) the firm receives 6,000 Lira, and the expected transfer to the manager is the expected cost of 4,500 Lira plus the manager's salary of 250. Alternatively, if the manager reported 5.00 to maximize his wealth ($\pi = 0\%$), then the firm would transfer 5,000 Lira and a salary of 250 to the manager. The second term, ($.5\{-250\}$), shows that when the actual cost is greater than 5.00, there is no production and the manager receives a salary of 250. The final expression for expected profit, $250 + 250\pi$, shows that for any value of $\pi > 0$ the expected profit under the MTC will be greater than 250, which is the expected profit under the HC.

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Why Does Fixation Persist? Experimental Evidence on the Judgment Performance Effects of Expensing Intangibles

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ABSTRACT: This study shows experimentally that when individuals use information on intangibles expenditures to predict future profits, expensing (vs. capitalizing) the expenditures significantly reduces the accuracy, consistency, consensus, and self-insight of individuals' subjective profit predictions. The experimental design allows us to eliminate several competing explanations for this apparent fixation on accounting. Subjects do not base their judgments on a naïve prior belief that expensing precludes effects on future profits; a pre-experiment question shows that subjects expect intangibles expenditures *will* affect future profits even when expensed. Moreover, subjects do not lack, or fail to use, data that would allow them to learn the exact expenditure-profit relation. They receive data on intangibles expenditures and profits as a basis for learning, and in some respects the learning is quite successful even when intangibles are expensed; subjects' profit predictions accurately reflect the mean and standard deviation of actual profits. Nevertheless, consistent with psychological theories of learning, subjects do not learn the exact magnitude of the effect of intangibles on future profits as well when the intangibles are expensed. Although the mean of their predictions is accurate, they do not discriminate well between cases with high and low actual profits. In consequence, their prediction accuracy, consistency, consensus, and self-insight are lower when intangibles are expensed. Thus, in this case, learning does not mitigate fixation on accounting, because accounting affects the learning process itself.

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I. INTRODUCTION

Both managerial and financial accounting research examine whether individuals fixate on accounting or can "see through" or "unscramble" the effects of alternative accounting methods (see reviews by Wilner and Birnberg 1986; Libby et al. forthcoming; Kothari forthcoming). Our study addresses a key question that remains unresolved in prior studies: Will opportunities to learn eliminate fixation?

A number of studies, both archival and experimental, suggest that fixation results from lack of experience or relevant data, and therefore opportunities to acquire this experience or data—i.e., to learn—should eliminate it (Chen and Schoderbek 2000; Gupta and King 1997; Waller et al. 1999). Experiments in which student subjects have opportunities to learn sometimes reduce or eliminate fixation on accounting (Gupta and King 1997; Waller et al. 1999). Professional financial analysts, however, continue to display fixation when predicting stock prices based on accounting information, although they presumably have opportunities to learn about the relation between accounting data and stock prices on the job, before participating in the experiment (Hopkins 1996; Hirst and Hopkins 1998; Hopkins et al. 2000). The absence of learning opportunities for financial analysts *within* the experiments remains problematic, however, especially because some of the accounting methods on which the experiments focus are relatively rare or novel (Lipe 1998).

In this study we test whether capitalization vs. expensing of intangibles expenditures results in fixation even when individuals have opportunities to learn. Accounting for intangibles is a particularly valuable context for testing the persistence of fixation. First, it has broad practical implications for both financial and managerial accounting. Aboody and Lev (1998) and Chan et al. (1999) suggest that requirements to expense intangibles for external reporting result in mispricing of some firms' stock. Even though GAAP requires firms to expense most expenditures for intangibles, a number of firms capitalize these expenditures for internal reporting out of concern that expensing can mislead managers (Stewart 1991; Tully 1993, 1998).

Second, individuals with business training and experience might reasonably be expected to see through the expensing of intangibles. Expensing vs. capitalizing intangibles is more widely publicized and conceptually simpler than most of the other accounting issues for which fixation has been demonstrated: debt-equity swaps (Hand 1990), deferred tax asset adjustments (Chen and Schoderbek 2000), accounting for mandatorily redeemable preferred stock (Hopkins 1996), and reclassification adjustments in comprehensive income for unrealized gains or losses in marketable securities (Hirst and Hopkins 1998; Maines and McDaniel 2000). By choosing a simple and familiar accounting issue, we provide a setting in which learning *not* to fixate should be relatively easy.

Subjects in our experiment receive data on intangibles expenditures (spending on a quality-improvement program) and gross profits at 20 similar manufacturing plants. In these data, the effect of the intangibles expenditures on profits in the current and two succeeding periods is too small to be statistically significant, but the effect on profits three periods in the future is large and statistically significant. The experiment tests whether subjects learn the lagged effect of quality-improvement expenditures on profits from these data, and whether they learn it equally well when the firm capitalizes expenditures (classifies them as investments in assets) or expenses them. The statistical predictive ability of the expenditure data is identical whether the expenditures are expensed or capitalized, and subjects' incentives to learn the relation are identical in both conditions.

When the firm capitalizes expenditures (investment condition), subjects learn the relation between intangible expenditures and profits relatively well. With expensed expenditures (expense condition), subjects do not learn the relation as well; on average, they underestimate the strength of the lagged relation by about half and make significantly greater prediction errors than do subjects in the investment condition. When they use what they have learned about the effect of intangibles on profits to predict new cases, subjects in the expense condition predict less consistently across cases and exhibit less consensus across individuals than do subjects in the investment condition. Moreover, subjects in the expense condition display less insight into their own judgment processes when asked to explain how they made their predictions.

These results do not occur because subjects naively believe that expensing means the expenditures have no future benefits. We verify in a pre-experiment question that all subjects expect intangibles expenditures to affect profits in future periods, even when the firm immediately expenses the intangibles. Moreover, nearly all subjects in the expense condition detect the three-period lagged effect of expenditures on profits in the data they receive. Compared to subjects in the investment condition, however, they make greater errors in estimating the *magnitude* of the lagged expenditure-profit effect, and they incorporate it much less effectively into their judgments, as their lower judgment consistency, consensus, and self-insight demonstrate.

These results show that learning is not necessarily a quick remedy for fixation on accounting, *because accounting can influence the learning process itself.* This finding is consistent with psychological theories of the learning process presented in Section II. This study adds to the repertory of explanations of how fixation occurs—and even persists despite apparent user sophistication about accounting (i.e., subjects' awareness of the potential future benefits of intangibles) and opportunities to eliminate fixation through learning.

The remainder of the paper proceeds as follows: Sections II through IV present the hypothesis motivation, design, and results of an experiment that tests the effects on judgment of capitalizing vs. expensing expenditures on intangibles. Section V discusses the study and its implications for future research.

II. HYPOTHESIS MOTIVATION

A wide range of individuals in an organization, from top management to lower-level employees with small-scale spending authorization, make decisions about expenditures on intangibles. Individuals' judgments about the effects of intangibles expenditures on profits are key inputs into these decisions. This section describes what individuals must learn to perform well in this judgment task, how they learn it, and how we expect accounting classification to affect the learning process.

What Must Individuals Learn?

Expenditures on intangibles affect current and future profits through two pathways, which we designate the *accounting-calculation effect* and the *indirect economic-causal effect.* As an example of the accounting-calculation effect, if the firm expenses a $10,000 expenditure on employee training, then the expenditure reduces current profits by $10,000 and has no accounting-calculation effect on future profits. If the firm capitalizes the expenditure and amortizes it over four periods, it reduces profits by $2,500 each period. Knowledge of basic accounting rules enables individuals to identify the magnitude and timing of these accounting-calculation effects.

The effect of training-program expenditures on profits includes not only these simple calculations, but also changes in revenue and/or operating costs resulting from changes in employee behavior after the training. When the expenditure yields a positive return, the indirect economic-causal effect[1] exceeds the accounting-calculation effect, and errors in estimating the economic-causal effect can affect profit predictions significantly. Individuals may develop prior beliefs about the magnitude and timing of indirect economic-causal effects based on their experience with other training programs or information about others' experience. However, individuals can estimate the actual profit effects of particular expenditures only by observing the relevant expenditures and profits, and then inferring the relation between them from these observations. Neither prior beliefs nor knowledge of accounting rules necessarily yield the same results as estimation of these indirect economic-causal relations from the data.

Field research has documented that managers use accounting reports to modify and refine their beliefs about the relations between their actions (e.g., expenditure choices) and profits. In a large-scale field study of nonfinancial managers' use of accounting data, McKinnon and Bruns (1992, 206) found that managers use periodic accounting reports to test and modify their mental models of key causal relations underlying firm performance.

> As managers review their success as reported in accounting reports, they are continuously at work, testing and perfecting their mental model of the relationship between activities and success as measured by the management accounting system....In this way, part of the accounting model is incorporated by managers into their own models. Managers learn to associate actions with organizational performance and success.

This observation is consistent with the claim that managers can use reports from performance measurement systems to test and modify the beliefs about cause-and-effect relations embedded in a firm's strategy and action plan—for example, the relation between employee-training expenditures and profits in a human-capital-intensive firm (Kaplan and Norton 1996a, 65).

Regressions of profits on expenditures capture both accounting-calculation and indirect economic-causal effects of these expenditures simultaneously. Although managers sometimes use such formal statistical analysis, subjective estimation is common in practice (McKinnon and Bruns 1992; Kaplan and Norton 1996b).[2] Such subjective estimation is a difficult inferential task, however, especially when individuals must assess lagged effects such as the effect on future profits of spending on quality, customer satisfaction, research and development, or employee training. Prior experimental research provides evidence that, as time lags between reported cause and effect increase, individuals are less able to detect causal relations and use them in judgments and decisions (Sterman 1989a, 1989b; Diehl and Sterman 1995). Little is known, however, about how accounting affects individuals' ability to detect lagged relations and use them appropriately in judgment.

Accounting and the Learning Process

This subsection uses basic psychology research about learning under uncertainty to predict how accounting classifications affect the learning process. We expect this effect to occur under the following two conditions only: (1) detecting expenditure-profit relations in the data and using them appropriately in judgment requires considerable cognitive processing effort: the relations are not so strong or salient that they are immediately transparent;

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[1] We call the economic-causal effects of expenditures "indirect" because they occur only through some intervening process. An employee training expense decreases current profit through the accounting-calculation effect, regardless of whether the training has any effect on employee behavior; but the expense affects future profit only through the intervening process of behavior changes.

[2] Individuals may use subjective approaches because they believe statistical analysis is too costly, the data quantity or quality is not sufficient to support reliable formal statistical analysis, or they believe they can outperform statistical models.

(2) the amount of potentially relevant data available is large enough, relative to individuals' cognitive information-processing resources, that not all data receive the maximum possible processing. In other words, the setting is such that attention and subjective processing effort are scarce resources (Birnberg and Shields 1984; Simon 1990).

These conditions have two possible alternative consequences. One is that individuals will spread limited attention roughly equally across all potentially relevant relations in the data, resulting in learning that is imperfect, but no more imperfect for one relation than for another. The second possibility is that individuals will allocate attention ***unequally*** across potentially relevant relations in the data; thus, they may not learn equally well relations that are equally strong in the data. Research in psychology, summarized below, provides evidence consistent with the second view.

Suppose individuals must estimate the relation between Y (e.g., current profits) and multiple X_i's (expenditures on a particular type of intangible in current and prior periods). Any one, or all, of the X_i's may affect Y, but the existence and magnitude of the relation are uncertain *ex ante*. In subjective estimation (unlike multiple regression), individuals tend to examine $X_i - Y$ relations one at a time rather than simultaneously, probably because of limited working memory (Brehmer 1979; Klayman 1988). Individuals use a variety of subjective strategies to estimate $X_i - Y$ relations, some more effort-intensive and more nearly optimal than others (Hutchinson and Alba 1997). In particular, individuals tend to allocate more attention and use more effort-intensive processing for the $X_i - Y$ relations they examine earlier; they tend to give less scrutiny to $X_i - Y$ relations that they examine later, resulting in significantly larger estimation errors (Brehmer 1974, 1979; Klayman 1988). The order in which individuals examine $X_i - Y$ relations is therefore key to judgment accuracy.

The order in which individuals examine $X_i - Y$ relations often depends on domain knowledge (prior beliefs) activated by data labels or referents (Muchinsky and Dudycha 1975; Sniezek 1986; Broniarczyk and Alba 1994). For example, suppose Y is total costs, X_1 is customer satisfaction, and X_2 is productivity. If individuals believe that productivity has a strong effect on total costs and that customer satisfaction has a weak effect, then they are likely to examine the productivity-total cost relation first and more carefully, and therefore to estimate it more accurately than the customer satisfaction-total cost relation. When individuals are unsure of the $X_i - Y$ relation in the data, perhaps because they have not examined the data intensively and lack strong prior beliefs about the relation, they tend to systematically underweight X_i, rather than systematically overweight it or weight it in a random but unbiased way (Sniezek 1986; Broniarczyk and Alba 1994). These processes are partly conscious and deliberately controlled, but also partly unconscious (Brehmer 1979; Broniarczyk and Alba 1994).

We expect individuals to attend earlier and more intensively to the lagged expenditure-profit relation when the firm capitalizes rather than expenses the expenditure. First, individuals may interpret a firm's decision to expense vs. capitalize an intangibles expenditure as a signal about the expected timing of the benefits from the expenditure, and may therefore deliberately direct more attention to lagged relations.[3] Second, the expense classification may automatically direct attention to the current-period relation, and individuals may therefore examine lagged relations less closely without being aware how unequally they are allocating their attention. In addition, the expense classification suggests a negative effect on profits; when individuals expect the wrong sign for a relation, they often estimate the magnitude of the relation less accurately (Muchinsky and Dudycha 1975; Sniezek 1986).

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[3] This argument assumes that individuals believe that information they do not possess has influenced the firm's choice between capitalizing and expensing the intangible expenditure.

Subjects in our experiment receive data on gross profits and expenditures on a quality-improvement program (employee training, etc.) at 20 similar manufacturing plants. We ask them to use these data to learn how quality-program expenditures affect gross profits. We then ask them to use what they have learned to predict gross profits at an additional 20 plants, given information on quality-program expenditures at these plants. The expenditures are classified either as expenses or as investments in assets. When the expenditures are classified as expenses, we expect individuals to allocate less attention to lagged effects; therefore they should learn this relation less accurately and be less certain of the strength of the lagged effect on profit, and therefore should tend to underestimate it. Because the lagged effect is an important determinant of profits in our experimental setting, we expect individuals' profit predictions to be less accurate when the intangibles are classified as expenses.

H1a: Individuals' profit predictions will be less accurate when expenditures on intangibles are expensed than when they are capitalized.

Our experimental design allows us to partition individuals' prediction errors into several components that can have different causes and different practical consequences. Appendix A shows a two-step breakdown of mean squared prediction error (MSE). The first step is the partition shown in Panel A (Theil 1966; Lee and Yates 1992):

$$\text{MSE} = (\overline{Y}_s - \overline{Y}_e)^2 + (S_{Y_s} - S_{Y_e})^2 + 2(1 - r_a)S_{Y_s}S_{Y_e} \qquad (1)$$

where $\overline{Y}_s$ (s = subject) is the mean of individuals' profit predictions, $\overline{Y}_e$ (e = environment) is the mean of actual profits, S_{Y_s} and S_{Y_e} are their corresponding standard deviations, and r_a is the Pearson correlation between the predicted and actual profits.

The three components of the partition represent different judgment errors. The first is *bias*: overall optimism or pessimism in predictions. If classifying the expenditures on intangibles as investments causes individuals to predict higher profits on average, regardless of the exact magnitude and timing of the quality expenditures, then the bias measure will capture this effect.

The second component of the partition is *variability*. Individuals' judgments may vary more or less around the mean than the actual outcomes do, perhaps because individuals tend to overreact or underreact *generally* to information provided. For example, an individual who uses the mean value of profits as the prediction for each case would exhibit no bias but extreme underreaction to the information available. If individuals expect quality expenditures to have more (or less) effect on profits when they are classified as investments, regardless of the timing of the expenditures, then the variability measure will capture this effect.

The third component is a function of r_a, which the psychology literature calls *achievement* (Lee and Yates 1992; Cooksey 1996), magnified by variability in actual and predicted outcomes. If individuals fail to identify the *relative* importance of different lags in the expenditure-profit relation or fail to use this knowledge consistently in their predictions—for example, if individuals expect contemporaneous effects of intangibles expenditures to be large and lagged effects to be small when in fact the converse occurs—then r_a will capture this effect.

We predict fixation will persist in this experiment because we expect individuals in the expense condition to underestimate the strength of the lagged effect of intangibles expenditures on profits, relative to individuals in the investment condition. This implies that r_a

will be smaller when intangibles are expensed, and that the difference in r_a across experimental conditions will be the principal source of difference in prediction error.

> **H1b:** Individuals' profit-prediction achievement (r_a) will be lower when intangibles are expensed than when they are capitalized.

Our explanation of expense vs. investment effects does not imply that individuals' predictions will be more optimistic or pessimistic on average or more (or less) responsive to all intangible-expenditure information. Therefore we do not hypothesize differences in the bias or variability of individuals' profit predictions.

Panels B and C in Appendix A show how to decompose r_a further to identify sources of difference in achievement. This decomposition, the so-called lens model (Tucker 1964; Lee and Yates 1992), is based on comparisons between two regression models of the relation between intangibles expenditures and profits. Regressing *actual profits* on actual expenditures, using the data that individuals receive to learn the expenditure-profit relation, creates an environmental model (Equation [2], Panel B). Regressing an *individual's profit predictions* on the expenditures individuals receive as a basis for prediction creates a policy-capturing model for that individual. If the individual's subjective analysis of the learning data leads to the same inferences about expenditure-profit relations as a statistical (regression) analysis, and if the individual consistently applies these inferences in making predictions, then the two models will be the same.

The lens-model equation below identifies potential sources of low achievement (r_a) in terms of correlations between the environmental and policy-capturing models.

$$r_a = R_e G R_s + C(1 - R_e^2)^{1/2} (1 - R_s^2)^{1/2}. \tag{3}$$

Panel C of Appendix A shows how we calculate the components of this equation. R_e (environmental predictability) measures the accuracy with which a statistical model predicts profits, using the data available to individuals. If the relation between profits and intangibles expenditures is deterministic, then R_e equals 1.0. The less of the variation in actual profits that the intangibles-expenditure data can explain, the lower R_e is.[4] G, which the psychology literature refers to as *matching* (Lee and Yates 1992; Cooksey 1996), captures the similarity in the relative magnitudes of the coefficients between an individual's policy-capturing model (Equation [3], Appendix A) and the environmental model (Equation [2], Appendix A).[5] R_s (*consistency* or cognitive control) captures the degree to which individuals use the same model without error from prediction to prediction. If unmodeled relations exist in the underlying data (e.g., if nonlinearities or interactions exist in the data but the statistical model employed is linear additive), then C captures the extent to which individual judgment incorporates these relations.

In our study, R_e is identical across conditions by design, and no nonlinear relations exist in the underlying data ($C = 0$). Therefore only two of the lens-model measures, matching (G) and consistency (R_s), can differ across conditions. We predict that expensing the intangibles expenditures will reduce matching, because individuals will underweight the lagged effect of intangibles on profits to a greater degree in the expense than in the investment condition.

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[4] R_e is the square root of the environmental model's unadjusted R^2.

[5] Because G is a correlation measure, it does not capture differences between the models with respect to intercepts or to absolute magnitudes of the coefficients. If an individual's policy-capturing model is $\hat{Y}_s = 5 + 10X_1 + 5X_2$ while the environmental model is $\hat{Y}_e = 1 + 2X_1 + X_2$, then predictions from the two models would be perfectly correlated and G would be 1.0, even though the individual's predictions are quite inaccurate. The inaccuracy would be fully captured in the first two components of the MSE partition, however, and therefore would not also be included in r_a or its components. (The individual's predictions would display optimism in the bias measure and overreaction in the variability measure.) In contrast, if the individual's policy-capturing model is $\hat{Y}_s = 5 + 1X_1 + 2X_2$—that is, if the relative weights on X_1 and X_2 differ from those in the environmental model—G would be substantially lower.

H2a: Matching (G) in individuals' profit predictions will be lower when intangibles are expensed than when they are capitalized.

H2b: In predicting profits, individuals will underweight the significant lagged effect of intangibles expenditures more when these expenditures are expensed than when they are capitalized.

Matching is important in practice because it indicates an understanding of the relative magnitudes of contemporaneous and lagged effects of intangibles expenditures. Consistency (R_s) is also important in practice, for two reasons. First, individuals' judgments about the effect of various expenditures on future profits often affect resource allocation decisions. Inconsistency in these judgments can impair resource allocation even if *average* judgment (as captured by G) is good. For example, individuals who make inconsistent judgments will sometimes overestimate future profits from a given type of expenditure and therefore overspend, while at other times they will underestimate future profits from the same expenditure and thus underspend. Second, efficient contracting requires the contracting individuals to be predictable: principals can design contracts to induce agents to take the actions the principals desire only if principals can predict how agents will respond to the incentives offered (Baiman 1982, 1990; Sunder 1999). Our consistency measure is an indicator of predictability. Thus, when individuals' judgments (and the actions that depend on these judgments) are inconsistent, contracts will not necessarily have the effect principals intend.

Sniezek (1986) shows that individuals make more consistent judgments when the label or referent of the data (in our case, "investment" vs. "expense") prompts them to examine important relations in the data early in the judgment process. In contrast, when individuals focus most of their attention on a relation that proves to be unimportant, they are likely to feel uncertain that they have a good basis on which to make judgments (profit predictions). They may therefore try multiple judgment strategies across cases, resulting in inconsistent judgments. In our setting, this is likely to occur when individuals allocate more attention to looking for a current-period expenditure-profit relation that is, in fact, a poor basis for profit prediction. In contrast, when they allocate more attention to learning the more predictive lagged effect, they should be more certain they have a good basis for profit prediction and thus should be more likely to use that basis consistently, rather than alternating among different judgment strategies. In consequence, we expect judgments to be less consistent when intangibles expenditures with future value are expensed than when they are capitalized.

H2c: Individuals' profit predictions will be less consistent (lower R_s) when intangibles are expensed than when they are capitalized.

We also expect the accounting classification to affect consensus and self-insight. Consistent with prior literature (Ashton 1985; Stewart et al. 1997), we define *consensus* as similarity in predictions across individuals. Low judgment consensus implies time-consuming discussion (and perhaps additional data collection and analysis) when individuals must establish agreement before making expenditure decisions. Low consensus can also imply costly conflict and, in the extreme, impasse and failure to make expenditures that would benefit the organization.

We expect the investment classification to lead to higher consensus. We expect individuals in the investment condition to be more successful in detecting and using the strong lagged effect of intangibles expenditures on profits, which is present in the data all individuals see. This common basis for prediction should lead to higher consensus. In the expense condition, in contrast, we expect individuals to be less successful in finding a satisfactory basis in the data for predicting profits. They must rely more on idiosyncratic prediction strategies and prior beliefs that vary across individuals, resulting in lower judgment consensus.

H3: Individuals' profit-prediction consensus will be lower when intangibles are expensed than when they are capitalized.

Consistent with prior literature, we define *self-insight* as the degree to which individuals' *ex post* explanations of how they made judgments correspond to how they actually made judgments (Cook and Stewart 1975). The lower individuals' self-insight, the less accurately they explain the basis of their judgments and therefore the more difficult and costly it is likely to be to resolve disagreements among individuals about the profits they expect from a given set of expenditures.

Self-insight can be low because only part of the judgment process is conscious. When individuals explain how they made judgments, part of their explanation is an account of their consciously directed thought processes, and part is their best guess about processes that are unobservable and/or difficult to recall accurately (Nisbett and Wilson 1977). Absent complete knowledge of their own judgment processes, individuals tend to report judgment processes they think would be reasonable under the circumstances (Nisbett and Wilson 1977).[6] Thus, if they believe an expense should have a strong effect on contemporaneous profits, then they will tend to report *ex post* that they weighted current-period expenditures heavily in the expense condition—even if they did not in fact do so. We expect that in the expense condition individuals will *not* in fact consistently place heavy weights on current-period expenditures. As predicted above, they will tend to be uncertain and to predict inconsistently. Thus when intangibles are expensed, individuals' actual use of information in predicting profits will correspond poorly with their *ex post* explanations. In the investment condition, in contrast, individuals are likely to believe an investment should have a strong lagged effect on profits, and therefore they will tend to report *ex post* that they weighted the lagged effect heavily in predicting profits. If, as predicted, they actually weight the lagged effect heavily in predictions, then correspondence between their actual and reported use of data will be high.

H4: Individuals' self-insight into their profit predictions will be lower when intangibles are expensed than when they are capitalized.

III. DESIGN OF EXPERIMENT

Subjects

Thirty-one M.B.A. students who had completed a course in management accounting and six undergraduates who had completed a course in cost accounting volunteered to participate in this experiment. *Full-time managerial* experience (not including other work experience) ranged from zero to ten years (mean of 35 months for M.B.A. students and 4 months for undergraduates). We asked subjects with managerial experience to estimate the percentage of time they had spent on budgeting (mean = 12 percent), forecasting and planning (32 percent), making spending decisions (15 percent), advising on or recommending spending decisions (12 percent), and quality programs (9 percent). Because t-tests revealed no significant ($p < 0.05$), differences between these two subject groups on the means of any of the variables collected (except for work experience and number of accounting and math courses completed), the tests described below pool data from both subject groups.[7] We paid subjects performance-contingent compensation, as described below.

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[6] Although the self-reports of judgment processes that Nisbett and Wilson (1977) recount appear to be sincere, some self-reports probably also reflect an element of impression management. Individuals may report weights that they believe will make them appear knowledgeable or rational.

Task

Subjects received information about 20 similar manufacturing plants. The plants made the same product and were built to the same design, using similar technology and production scale. All plants participated in a quality-improvement initiative, which included employee training, process and quality engineering, and preventive maintenance. Internal accounting reports classified spending on this program as either a quality-improvement expense or an investment (the experimental treatment). In the investment condition, the firm capitalized the expenditures and straight-line amortized them, deducting the amortization expenses in computing gross profits. In the expense condition, the firm subtracted the entire amount of the expenditures in calculating gross profits in the quarter during which the spending occurred.

An introductory page in the experimental materials told subjects that because the plants were so similar, the effect on quarterly gross profits of a dollar of spending on the quality-improvement program was roughly the same across plants. However, because the program was new, the firm was still learning how the program affected quarterly gross profits and what the optimal level of spending was. Local managers had some freedom to experiment with different quality-improvement spending levels. There were no seasonal variations in the data and no significant external shocks or unusual internal events that would have altered or masked the effects of quarterly quality-improvement expenses on quarterly gross profits. Therefore, *if* the quality-improvement program had a significant effect on quarterly gross profits, then it should be detectable in the data.

After reading this introduction, each subject received expenditure data for 20 plants for the just-completed quarter (t), expenditure data for the preceding three quarters ($t - 1$, $t - 2$, and $t - 3$), and actual gross profit for quarter t. This is the learning data set. Table 1 shows the learning data subjects received, which was identical across experimental conditions except for the word "expense" or "investment" in the column heading.

The advantage of providing identical expenditure and profit data to subjects in both conditions is enhanced experimental control; but the disadvantage is that the underlying

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[7] The groups differed in one respect consistent with the difference in experience: Prior beliefs about the effect of quality expenditures on profit were more diverse among undergraduate subjects. The variance in prior beliefs was significantly greater for undergraduates than for M.B.A.s (Levene's $F = 32.82$, $p < 0.00$ for prior beliefs about current-period effects; $F = 30.81$, $p < 0.00$ for prior beliefs about the sum of the next three periods' lagged effects). Although more experienced subjects exhibited more consensus *before* they saw the learning data, there was no difference across subjects groups in post-learning consensus. Variance in the $t - 3$ lagged coefficient was a principal driver of low consensus (see Table 4); but this variance was not significantly greater for undergraduates than for M.B.A.s ($F = 1.17$, $p > 0.05$).

Dependent Variables

We used each subject's gross profit predictions to develop the dependent variables. We measured judgment accuracy (H1a) for each subject as the mean absolute error of the subject's gross profit predictions:[10]

$$\Sigma_j|Y_{sj} - Y_{ej}|/20, \qquad j = 1,...20,$$

where:

Y_{sj} = subject s's profit prediction for plant j, and
Y_{ej} = actual profit for plant j.

Appendix A shows the calculation for judgment achievement (r_a), matching (G), and consistency (R_s), the dependent measures for H1b, H2a, and H2c, respectively. The measure for H2b was the difference between standardized coefficients on intangibles expenditures at t − 3 in the environmental model and subjects' policy-capturing models (standardized values of $b_{st-3} - b_{et-3}$ in the models shown in Appendix A).[11]

We used two measures of judgment consensus (H3). One, used frequently in lens-model studies (Ashton 1985; Ashton 1992; Stewart et al. 1997), was the mean of the Pearson correlations between the judgments of each pair of subjects in an experimental condition. The second was the variance of the coefficients in subjects' policy-capturing models (β_s's in Equation [2], Appendix A).

Also consistent with prior literature (Cook and Stewart 1975), we measured self-insight (H4) as the correlation between two sets of gross profit predictions: (1) predictions based on the policy-capturing model estimated for each subject ($\hat{Y}_s$ in Appendix A) and (2) predictions based on the weights that subjects supplied *ex post* when we asked how important each of the four periods of expenditure data was in making their predictions. If they correctly reported the relative impacts of the four periods of expenditure data on their predictions, the self-insight measure would equal 1.0. To the extent that the relative weights they reported differed from the relative weights in their policy-capturing models, the measure would be smaller.

Procedure

We randomly assigned each subject to one of the two treatment conditions (expense or investment). Subjects self-paced their way through the experimental materials in a lab setting. The first section of the materials, which subjects completed and returned before seeing the rest of materials, collected information on subjects' prior beliefs about the effect on gross profits of spending on a quality-improvement program in a manufacturing facility. The description of this program (which included employee training, process improvement, etc.) was the same as the description that appeared subsequently in the main task of the experiment. Subjects estimated the profit impact of a given level of spending on the quality program in the quarter of the expenditure and in each of several subsequent quarters. We told subjects in the investment condition that the expenditures were capitalized, and subjects in the expense condition that they were expensed.

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[10] We also calculated and tested mean squared prediction error (MSE), since the partitioning described in Section II applies to MSE, not to mean absolute error. See footnote 13 for results. We used mean absolute prediction error as the primary dependent variable because it was more nearly normally distributed and more interpretable.

[11] The psychology literature often uses standardized coefficients because they facilitate comparisons across judgment tasks with differently scaled predictors (Cooksey 1996).

TABLE 4

Comparison of Subjects' Policy-Capturing Models with: (1) Environmental Model (Matching) and (2) Subjects' Predictions (Consistency)

	Expense *(n = 18)* *Mean* *(Std. Dev.)*	*Investment* *(n = 19)* *Mean* *(Std. Dev.)*	*t-statistic* *(two-tailed p)*
Matching (G)	0.52 (0.57)	0.85 (0.17)	2.33 (0.03)
$\beta_{st} - \beta_{et}$	0.14 (0.29)	0.19 (0.28)	0.52 (0.61)
$\beta_{st-1} - \beta_{et-1}$	0.14 (0.23)	0.15 (0.20)	0.41 (0.69)
$\beta_{st-2} - \beta_{et-2}$	0.21 (0.29)	0.24 (0.24)	0.27 (0.79)
$\beta_{st-3} - \beta_{et-3}$	−0.48 (0.46)	−0.14 (0.22)	2.86 (0.01)
Consistency (R_s)	0.69 (0.26)	0.86 (0.15)	2.40 (0.02)

Matching (G) measures the similarity between subjects' policy-capturing models and an environmental model (see Appendix A for definitions of measures and models). $\beta_{st} - \beta_{et}$ is the difference between the standardized coefficient on contemporaneous intangibles expenditure in subjects' policy-capturing models and the corresponding coefficient in the environmental model (and similarly for t−1, t−2, and t−3). Consistency (R_s) measures the degree to which subjects use the same model without error in multiple predictions.

matching (G) was significantly lower in the expense (0.52) than in the investment condition (0.85; $t = 2.33$, $p < 0.03$), supporting H2a.

G measured the simultaneous effect of all four coefficients in each subject's policy-capturing model and did not indicate what was specifically wrong with subjects' models in the expense condition. Rows two to five of Table 4 show mean differences between the standardized coefficient (β_e) on each quarterly spending variable in the environmental model (Equation [1], Appendix A) and the corresponding standardized coefficient in subjects' predictive models (β_s) (Equation [2] in Appendix A) in each condition. The accounting classification of expenditures on intangibles did not affect the accuracy of subject-model coefficients on *contemporaneous* intangibles expenditures ($\beta_{st} - \beta_{et}$) ($t = 0.52$, $p < 0.61$), nor on expenditures at $t - 1$ and $t - 2$ ($t = 0.41$, $p < 0.69$ and $t = 0.27$, $p < 0.79$, respectively). Consistent with H2b, however, mean errors in subject-model coefficients for the three-period lag ($\beta_{st-3} - \beta_{et-3}$) were significantly greater when intangibles expenditures were classified as expenses (−0.48) than when they were classified as investments (−0.14;

$t = 2.86$, $p < 0.01$).[14] Expensing the intangibles caused subjects to significantly underestimate the strength of this important positive lagged effect.[15]

The last row in Table 4 shows results for consistency (R_s). As predicted in H2c, subjects' mean level of consistency was lower in the expense condition (0.69) than in the investment condition (0.86; $t = 2.40$, $p < 0.02$).

Hypothesis 3 predicted lower judgment consensus in the expense condition than in the investment condition. As Table 5 shows, mean pairwise correlations between subjects' predictions were lower in the expense condition ($r = 0.19$) than in the investment condition ($r = 0.58$). We did not perform statistical tests of this difference, however, because the correlations were not independent (each subject's predictions were correlated with the predictions of all other subjects in his or her experimental condition). Individual subjects' policy-capturing models provided an alternative measure of consensus. Variation in subjects' models (β_s's) created variation in predictions (lack of consensus). (Table 4 reports standard deviations of β_s's.) The variance of β_{st-3} was significantly greater in the expense condition than in the investment condition, indicating less consensus about the key predictor of profits in the expense condition (0.21 vs. 0.05; $F = 4.57$, $p < 0.01$). Variances of β_{st}, β_{st-1}, and β_{st-2} did not differ significantly across conditions: F's < 1.50, p's > 0.10. These results supported H3.

Hypothesis 4 predicted that self-insight would be lower in the expense condition than in the investment condition. The last row in Table 5 shows the self-insight measure, which captured the similarity between the relative weights on expenditure variables in subjects' policy-capturing models and the relative weights subjects provided *ex post*. Mean self-insight was significantly lower in the expense condition (0.47) than in the investment condition (0.85; $t = 2.08$, $p < 0.05$), consistent with H4.

Supplementary Analyses

The distributions of most of the judgment performance measures were somewhat skewed. We therefore performed nonparametric (Mann-Whitney U) tests of the hypotheses. Results did not differ qualitatively from the parametric results reported above. We also tested all hypotheses with a reduced sample, omitting the six undergraduate subjects, using both parametric and nonparametric tests. Results were qualitatively similar except for the nonparametric test of self-insight, which did not reach conventional levels of significance (one-tailed $p < 0.16$).

Prior Beliefs

We have argued that subjects' profit predictions differed in the expense and investment conditions because subjects processed the learning data differently in the two conditions. One potential alternative explanation was that profit predictions differed because subjects

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[14] Analysis of unstandardized coefficients led to qualitatively similar inferences. We also obtained qualitatively similar results with a MANOVA on all four coefficients, which allows for the potential within-individual correlations of the four coefficients.

[15] An alternative analysis pooled all subjects' profit predictions and estimated the following regression: $Y_{sj} = a + b_1X_{jt} + b_2X_{jt-1} + b_3\,X_{jt-2} + b_4X_{jt-3} + Z + b_5ZX_{jt} + b_6ZX_{jt-1} + b_7ZX_{jt-2} + b_8ZX_{jt-3}$, where Y_{sj} = profit prediction by subject s for plant j, X_{jt} = expenditure at plant j at time t, and $Z = 0$ in expense condition, 1 in investment condition. The adjusted R^2 of the pooled regression was 0.32 ($F = 37.74$, $p < 0.001$). The experimental condition significantly affected subjects' use of the expenditure data at $t - 3$; b_8 was significantly positive ($t = 3.90$, $p < 0.001$). Consistent with the analysis in Table 4, the experimental condition did not affect subjects' use of the expenditure data at t, $t - 1$, or $t - 2$; b_5, b_6, and b_7 were not significantly different from zero (t's < 0.60, p's > 0.54).

TABLE 5
Judgment Consensus and Self-Insight

	Expense (n = 18)	*Investment (n = 19)*	*Expense vs. Investment Tests*
Consensus: Mean pairwise correlations between subjects' gross profit predictions	0.19	0.58	NA
Consensus: Variance of β_{st-3}	0.21	0.05	$F = 4.57$ ($p < 0.01$)
Self-insight mean (std. dev.)	0.47 (0.60)	0.85 (0.18)	$t = 2.08$ ($p < 0.05$)

See Appendix A, Panel C for calculation of measures.

in the investment condition had prior beliefs more closely resembling the underlying relations in the learning data than did subjects in the expense condition. A related alternative was that subjects believed the accounting classification provided a credible signal about the magnitude and timing of the profit impacts of expenditures on quality improvement, and that this belief directly affected their profit predictions, independent of the way they processed the learning data. For example, subjects in the expense condition might have accurately estimated the three-period lagged effect in the learning data, but discounted it in judgment because of a belief that the accounting classification signaled a minimal lagged effect.

We controlled for both of these alternatives by performing analyses of covariance on the six key dependent measures: mean absolute error, achievement (r_a), matching (G), error in subjects' β_{st-3} coefficients, consistency (R_s), and self-insight. The covariates in these analyses were subjects' pre-experiment estimates of the contemporaneous and future effects on gross profits of quality-program expenditures, classified as either investments or expenses.[16] The covariates were at least marginally significant (one-tailed $p < 0.10$) in the analyses of mean absolute error, achievement, matching, and the error in the $t - 3$ coefficient, but not in the analyses of consistency and self-insight. The signs of the coefficients on the covariates were intuitively plausible. When subjects expected the *lagged* effect to be large, the $t - 3$ coefficients in their policy-capturing models were larger, their achievement and matching scores were higher, and their mean absolute error was smaller. When they expected the *current*-period effect to be large, their $t - 3$ coefficients were smaller, their achievement and matching scores were lower, and their mean absolute error was greater.

After controlling for the effects of these prior beliefs, however, the effect of the accounting classification remained significant (two-tailed $p < 0.05$) for each of the dependent variables examined. This result indicated that the judgment differences between expense and investment conditions were not due solely to prior beliefs about quality expense and investment, independent of what subjects inferred from the learning data set.

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[16] The future-quarter prior-belief estimate was the sum of the profit effects subjects estimated for the first, second, and third quarters after the expenditure. We obtained the same qualitative results using the estimated effect for the third quarter after the expenditure and the sum of all future quarters' estimated effects. Subjects' mean (standard deviation) estimate of the effect of \$1 of expenditure on the quality program was \$1.58 (\$8.44) for the current quarter and \$1.61 (\$6.71) for the next three quarters.

Attention Directing

The self-insight results suggested the limitations of retrospective reports, but also indicated that these reports were informative; the self-insight measure was significantly positive (one-tailed $p < 0.05$) for 32 of the 37 subjects. Subjects' retrospective reports of their judgment processes (see Appendix B) further supported our claim that the expense and investment classifications focused attention on different subsets of the learning data. The investment classification focused individuals' attention more on future-period effects (explanations 5–7 in Appendix B), whereas the expense classification focused their attention more on current-period effects (explanations 1–4 in Appendix B). On average, subjects in the investment condition assigned 28.2 *more* points to future-period explanations than to current-period explanations, while subjects in the expense condition assigned 12.5 *fewer* points to future-period than to current-period explanations. This difference in point assignment between investment and expense conditions was significant ($t = 2.91$, $p < 0.01$).

V. DISCUSSION AND FUTURE RESEARCH

Prior research has questioned whether individuals will learn to see through accounting when significant learning opportunities are available (Wilner and Birnberg 1986; Lipe 1998), but evidence on learning has been limited. Our experiment shows that opportunity to learn is not necessarily a quick remedy for accounting fixation, because accounting can affect the allocation of attention and thus can influence the learning process itself. We do not suggest that people are *incapable* of overcoming fixation and learning to make correct inferences from accounting data. With fewer competing demands on attention, additional cues to direct attention to important relations, or incentives that increase total attention, individuals might allocate enough attention to a lagged expense-profit relation to predict profits more accurately. However, all else equal, people can arrive at accurate predictions more quickly and easily when accounting is closer to economic reality. This means learning at lower cost in terms of data, attention, and effort (lower out-of-pocket compensation costs for effort and lower opportunity costs of directing attention away from other issues). In some settings there may also be competitive-advantage benefits to learning key economic relations more quickly.

Our results also contribute to understanding what it means to be a sophisticated user of accounting information. Early fixation studies see a lack of accounting-calculation knowledge as the reason for judgment biases: "If outputs from different accounting methods are called by the same name, such as profit, cost, etc., *people who do not understand accounting* will tend to neglect the fact that alternative methods may have been used to prepare the outputs" (Ijiri et al. 1966, 194, emphasis added; see Ball 1972, 1 for a similar argument). In this view, individuals should not fixate if they have sufficient knowledge of relevant accounting rules.

More recent studies have emphasized the importance of category knowledge about indirect economic-causal effects of accounting; for example, experienced analysts know that decreases in stock price typically accompany one category of financing (new equity issues) but not another (new debt issues) (Hopkins 1996). Individuals who possess this knowledge may appear more sophisticated than those who do not; but they are also *more* likely to display fixation when encountering an atypical instance of the category, such as a hybrid security classified as debt or equity (Hopkins 1996; Libby et al. forthcoming). Additional encounters with atypical instances could help individuals refine their category knowledge and deal appropriately with atypical members of a category, however. In this view, the sophistication required to avoid fixation on accounting would consist of refined category knowledge that clearly distinguishes atypical from typical instances and supports correct expectations about the behavior of atypical instances.

Our study suggests that the requirements of user sophistication can be even more demanding; a clear understanding of atypical instances in principle is not always sufficient to eliminate fixation in practice. Intangibles are an atypical expense in that they affect future-period profits, whereas typical expenses do not. Our subjects understand this, as shown in their responses to the pre-experiment question about the effect of an intangibles expense on future profits. Subjects in the expense condition expect a lagged effect on profits—but their performance in estimating this effect and using it in judgments is still significantly worse than that of subjects in the investment condition.

Subjects in the expense condition perform relatively poorly because they look less closely at the most important data, *not* because they draw erroneous conclusions from the data at which they do look closely. (Table 4 showed that subjects in the expense condition do *not* overestimate the current-period effects of the expenditures, compared to subjects in the investment condition.) This attention-allocation story may help to explain the findings of prior research about the effectiveness of learning opportunities in reducing fixation. For example, in Waller et al. (1999), fixation diminishes rapidly with opportunities to learn, perhaps because the experimental setting poses no significant attention-allocation problem. Subjects in that experiment receive cost information on a single product, choose a price for the product, and then learn the profit they make, which is a deterministic function of price. Thus, subjects in Waller et al. (1999) must learn a deterministic relation between one X (price) and one Y (profit), rather than a probabilistic relation between four $X_{i's}$ (four periods of expenditures) and one Y (profit), as in our experiment. The likelihood that subjects will not closely examine data on the right X_{i-Y} relation is presumably minimal when there is only one X_i.[17] Similarly, in Gupta and King (1997) subjects must learn to estimate costs for three products by observing the relation between their cost estimates (X_i) and aggregate profits (Y); the more accurate their estimates are, the higher their profits. Learning is slower when subjects must estimate three $X_i - Y$ relations (because the accounting system provides inaccurate data on all three) than when they must estimate only two (because the accounting system estimates one product cost correctly).

One potentially important difference between our experiment and Waller et al. (1999) and Gupta and King (1997) is the way we presented the learning data. Subjects in Waller et al. (1999) and Gupta and King (1997) decided on prices or cost estimates in *each* trial of the experiment before seeing the actual profit outcome. In the learning phase of our experiment, subjects learned profit outcomes of intangibles expenditures without having to make predictions first. Some psychologists have argued that requiring predictions or decisions during the learning phase should promote learning because it creates greater involvement; others have argued that it should reduce learning because it inhibits integration of learning data across cases or causes anchoring on initial judgments, which are likely to be inaccurate because they are made on the basis of very little data (Klayman 1988; Broniarczyk and Alba 1994; Well et al. 1988). Experiments have found, however, that requiring case-by-case predictions during the learning phase makes no difference to performance in the subsequent judgment phase, other things equal (Broniarczyk and Alba 1994; Well et al. 1988). It seems unlikely, therefore, that our experimental results are driven by the absence of prediction requirements during the learning phase.

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[17] Subjects actually choose a price-quantity combination in Waller et al. (1999), but quantity errors are rare; the learning that takes place in the study is predominantly reduction in pricing error.

Broniarczyk and Alba (1994) also find, however, that simultaneous presentation of all the learning cases (as in our experiment) leads to poorer learning than does a sequential presentation that requires subjects to examine each case for some time before they can see the next. These two presentation formats correspond to different real-world tasks—for example, examining reports from a dozen similar business units for a given period simultaneously vs. examining reports from a given unit over time. Our experimental setting corresponds to the first situation, and the results may not be fully generalizable to the second.

Another limitation to the generalizability of this paper's results is that our experimental setting does not include actions some firms take to aid potentially faulty judgment. Our subjects made their judgments alone; the opportunity to discuss the data with others might mitigate or exacerbate the performance deficits observed in the expense condition. Our subjects also make their judgments without the aid of formal statistical methods that might help overcome judgment biases. Task-properties feedback leads to better judgment performance than outcome information alone (Bonner and Walker 1994); and in tasks like ours, task-properties feedback is defined as statistical information such as correlations and regression coefficients (Balzer et al. 1989). Some firms provide statistical models of the drivers of financial performance to employees as substitutes for subjective estimation, but many do not; the efficacy of statistical models is a contested issue (Kaplan and Norton 1996b; Ittner and Larcker 1998), and individuals' willingness to rely on such models instead of their own subjective judgment is an open question.

When individuals make subjective estimates, other attention-directing devices can counter accounting methods' tendency to lead managers to misallocate attention. For example, additional nonfinancial data may prompt closer examination of lagged relations by drawing attention to the link from expenditure at one point in time to increased revenues or decreased operating costs at later times by way of intermediate-period improvements in quality, productivity, customer satisfaction, etc.

The usual limitations of laboratory research suggest caution in assuming that our results will replicate fully in natural settings. The task and data are simplified and stylized, and some subjects have limited work experience—although many have relevant experience, and experience differences did not drive results. Sufficient specialized experience can provide knowledge about intangibles that eliminates the judgment task represented in this experiment; that is, it can create settings in which there is no longer significant *ex ante* uncertainty about the timing of benefits from intangible expenditures. In many important settings, however, individuals lack this specialized knowledge because they are facing new situations and must learn from observing available data. For many expenditures on "softer" intangibles such as employee training and process improvement, it is far from clear how soon firms should expect to reap benefits from a particular new initiative, or how long additional benefits will persist. Even experienced managers depend on accounting data to infer the timing and magnitude of these effects.

APPENDIX A
Prediction Error and Lens Model Statistics

Panel A: Partition of Mean Squared Prediction Error

$$\text{MSE} = (\bar{Y}_s - \bar{Y}_e)^2 + (S_{Y_s} - S_{Y_e})^2 + 2(1 - r_a)S_{Y_s}S_{Y_e} \tag{1}$$

where:

$\text{MSE} = \Sigma_j(Y_{sj} - Y_{ej})^2/20,\ j = 1,\ldots,20;$
Y_{sj} = subject s's gross profit prediction for plant j;
Y_{ej} = actual (environmental) gross profit for plant j;
$\bar{Y}_s = \Sigma_j Y_{sj}/20;$
$\bar{Y}_e = \Sigma_j Y_{ej}/20;$
S_{Y_s} = standard deviation of Y_s;
S_{Y_e} = standard deviation of Y_e; and
$r_a = r(Y_e, Y_s)$.

Panel B: Modeling the Environment and the Individual

Learning data subjects received
Y_{ei} = actual (environmental) gross profit at plant i in quarter t, i = 1,...,20.
$X_{it}, X_{it-1}, X_{it-2}, X_{it-3}$ = quality expenditure at plant i in quarter t, t − 1, etc.

Judgment data subjects received
$X_{jt}, X_{jt-1}, X_{jt-2}, X_{jt-3}$ = quality expenditure at plant j in quarter t, t − 1, etc., j = 1,...,20.

Predictions and outcomes
Y_{sj} = subject s's prediction of gross profit for plant j in quarter t.
Y_{ej} = actual (environmental) gross profit for plant j in quarter t.

Environmental model
Estimated by regressing Y_{ei} on X_{it}, X_{it-1}, X_{it-2}, and X_{it-3}:

$$\hat{Y}_e = a_e + b_{1e}X_t + b_{2e}X_{t-1} + b_{3e}X_{t-2} + b_{4e}X_{t-3}. \tag{2}$$

Individual policy-capturing model
Estimated separately for each subject by regressing Y_{sj} on X_{jt}, X_{jt-1}, X_{jt-2}, and X_{jt-3}

$$\hat{Y}_s = a_s + b_{1s}X_t + b_{2s}X_{t-1} + b_{3s}X_{t-2} + b_{4s}X_{t-3}. \tag{3}$$

Panel C: Lens Model Equation and Measures

$$r_a = R_eGR_s + C(1 - R_e^2)^{1/2}(1 - R_s^2)^{1/2} \tag{4}$$

where:

r_a (achievement) = $r(Y_e, Y_s)$;
R_e (environmental predictability) = $r(Y_e, \hat{Y}_e)$;
G (matching) = $r(\hat{Y}_e, \hat{Y}_s)$;
R_s (consistency) = $r(Y_s, \hat{Y}_s)$;
C (residual achievement) = $r(d_{Y_e}, d_{Y_s})$;
$d_{Yej} = Y_{ej} - \hat{Y}_{ej}$; and
$d_{Ysj} = Y_{sj} - \hat{Y}_{sj}$.

Additional measures

$$\text{Self-insight} = r(\hat{Y}_s, \hat{Y}_w),$$

where $\hat{Y}_w = b_{1w}X_t + b_{2w}X_{t-1} + b_{3w}X_{t-2} + b_{4w}X_{t-3}$ and b_w is the weight provided by subject s in a post-experiment question.

$$\text{Consensus (pairwise)} = r(Y_{sm}, Y_{sn}),$$

where m and n are individual subjects, $m \neq n$. Pairwise correlations are averaged to provide consensus measures for larger groups.

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[a] Note that, consistent with prior literature (Libby 1981; Lee and Yates 1992; Cooksey 1996), we use the *judgment* data to estimate subjects' policy-capturing models, but use the *learning* data to estimate the environmental model. Although subjects *reveal* their policy-capturing models through use of the judgment data, they *estimate* their models from the learning data. The appropriate measure of the quality of their subjective estimation is therefore a statistical model estimated from the learning data.

APPENDIX B

Retrospective Thoughts about How Quality-Improvement Expenditures Affect Profits

Please try to remember how you thought quarterly quality-improvement expenses affect quarterly gross profits **when you made your quarterly gross profit predictions.** Allocate 100 points across the following factors, based on **how important you thought they were** when you made your predictions. Enter zero for any explanations that did **not** occur to you when you were making your predictions. *[Mean responses for expense (E) and investment (I) conditions are shown in the two right-hand columns.]*

	Means	
	E	*I*
1. Increased quality-improvement expenses in the current quarter **decrease profits in that quarter** because they are subtracted from revenues in the profit calculation.	14.6	13.7
2. Increased quality-improvement expenses in the current quarter **decrease** profits in **that quarter** because they cause temporarily disruptive and costly changes in products and/or operating processes.	14.6	9.5
3. Increased quality-improvement expenses in the current quarter **increase** profits in **that quarter** because improved quality makes the product more attractive to customers and increases that quarter's sales revenues.	12.8	1.8
4. Increased quality-improvement expenses in the current quarter **increase** profits in **that quarter** because they make the operating processes more efficient and reduce product costs that quarter.	13.9	1.6
5. Increased quality-improvement expenses in the current quarter **increase** profits in **future quarters** because the product gradually becomes more attractive to customers, as they become aware of the improvement.	20.0	20.3

6. Increased quality-improvement expenses in the current quarter **increase** profits in **future quarters** as the full effect of increased operating efficiencies is gradually realized.	19.2	31.1
7. Increased quality-improvement expenses in the current quarter **decrease** profits in **future quarters** because they take money away from other, more profitable investments your firm could make.	4.2	3.4
8. Other (explain):	0.8	18.2

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Earnings Dilution and the Explanatory Power of Earnings for Returns

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ABSTRACT: Executive stock options and convertible securities can increase the number of common shares outstanding while adding less than the market value of the newly issued securities to a firm's assets. We model the effect of expected dilution on the earnings/return relation. Expected dilution effectively reduces the permanence of an earnings innovation. Empirical evidence supports the hypothesis that dilutive securities attenuate the relation between earnings and returns. Estimated earnings response coefficients (ERCs) are significantly lower when there are shares reserved for conversion. The effect is more pronounced for firms that have experienced price increases or positive earnings news, as these increase the expected dilutive effect of conversions.

Keywords: *stock options; dilution; earnings; stock returns.*

I. INTRODUCTION

We examine whether the association between stock returns and earnings reflects the potential diluting effect of securities such as executive stock options, stock rights, convertible preferred shares, and convertible bonds. The increased use of these dilutive securities—in particular, of executive stock options—has attracted the attention of the business press, regulators, and academics. In May 1998, for example, the *Forbes* magazine cover stated "Stock Options are Diluting Future Earnings." The related story (Morgenson 1998) detailed firms' increasing use of options as a form of executive compensation, stated that option plans represent a mortgage on future earnings, and implied

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that investors are largely unaware of the cost associated with options plans. This last concern exists despite the fact that accountants have tried to incorporate the effect of dilutive securities into accounting numbers since 1969, when the Accounting Principles Board passed APB No. 15 (AICPA 1969).

We explore how current shareholders incorporate unexpected earnings into share price when the firm has outstanding dilutive securities. We extend Ohlson (1995) and show analytically that investors should place lower value on unexpected earnings when they anticipate dilution. We then investigate the relation among earnings changes, returns, and dilutive securities in an association study framework. We find that expected dilution significantly weakens the relation between contemporaneous earnings changes and returns. This result holds whether we define earnings changes as changes in total earnings, changes in primary earnings per share (EPS), or changes in fully diluted EPS. We also find that shareholders perceive expected dilution to be more costly for firms with rising stock prices and increasing earnings, because these conditions make the exercise of dilutive securities more attractive to their holders. These results suggest that, contrary to the opinions of the business press, investors do not ignore the costs of option issuance. However, our findings indicate that existing GAAP-based measures of diluted EPS are not conservative enough to capture the full effect of dilutive securities.

Recent academic literature has expressed concern over accounting measures of dilution—in particular, the use of the treasury stock method to incorporate the dilutive effect of options in the EPS calculation. APB No. 15 first mandated the treasury stock method in 1969; although Statement of Financial Accounting Standards (SFAS) No. 128 (FASB 1997) supersedes APB No. 15, it continues to use this method.[1] The treasury stock method assumes that the firm uses the hypothetical receipt of the exercise price from option holders to purchase shares on the open market at the average market price for the year. Accounting dilution occurs when the exercise price is less than the average market price. In this case, the shares deemed purchased are insufficient to satisfy all of the options, and the difference is made up through the issue of hypothetical new shares that are added to the denominator of the diluted EPS calculation.

Scott and Wier (2000) examine the effect of treasury stock-based dilution adjustments on the properties of primary EPS. They point out that the treasury stock method of incorporating dilution into the EPS number results in a mechanical, positive relation between the change in the number of shares added to the denominator of EPS and current stock returns. This positive association between changes in dilution and stock returns does not mean that investors like increases in dilution. Rather, as Scott and Wier (2000) point out, it results from the use of the price-based treasury stock method in the presence of accounting conservatism.[2]

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[1] SFAS No. 128 modifies APB No. 15 in several respects, the most major change being the shift from the calculation of primary EPS, which incorporated "expected" dilution, to the calculation of basic EPS, which does not incorporate dilution into the EPS calculation. The incorporation of maximum potential accounting dilution in diluted EPS remains largely unchanged relative to the calculation of fully diluted EPS mandated in APB No. 15.

[2] Under certain simplifying assumptions, Scott and Wier's (2000) change in the dilutive adjustment simplifies to $(\Delta N_t^*/N_{t-1}^*) \times (\text{primary EPS}_t)$, where ΔN_t^* is the change in the number of shares entering the denominator of primary EPS under the treasury stock method, and N_{t-1}^* equals the number of shares in the denominator of last period's primary EPS. ΔN_t^* increases in the change in the market price of the firm's common stock and in the number of dilutive shares outstanding. Scott and Wier (2000) provide empirical evidence that the positive relation between ΔN_t^* and share price changes leads to a positive relation between the annual *change* in the firm's dilutive adjustment and contemporaneous stock returns.

Scott and Wier (2000) also report that the correlation between earnings changes and returns is higher when they make no dilutive adjustment to earnings. Combining this evidence with their observed positive association between returns and the *change* in the dilutive adjustment, Scott and Wier (2000) conclude that the treasury stock method may be too conservative. This conclusion is premature for the following reasons. First, because treasury stock-adjusted earnings changes have a component that is proportional to unadjusted earnings changes plus a component that is inversely correlated with current returns (the change in the dilutive adjustment), it is not surprising that adjusted earnings changes have a lower correlation with current returns than do unadjusted earnings changes. Second, all of the treasury shares that the treasury stock method adds to the denominator of the EPS calculation are dilutive. Clearly, shareholders should care about *total* expected dilution, not just about the most recent *increment* to dilution, which is Scott and Wier's (2000) focus. Thus, the Scott and Wier (2000) results cannot address whether the total *level* of dilutive adjustment is too large or too small. Our current, more comprehensive analysis focuses on the expected level of dilution and is not affected by the mechanical relation between increments to the dilutive adjustment and returns that underlies Scott and Wier's (2000) result. Focusing on the levels demonstrates that shareholders view prospective dilution as costly, and our evidence suggests the treasury stock method may not be conservative enough.

Core et al. (2000) also suggest that the treasury stock method understates dilution because it considers only the degree to which options are currently in the money. They propose instead the use of option-pricing methods to calculate the denominator of the diluted EPS number. This increases the denominator of diluted EPS by the difference between the number of shares calculated using the option methods vs. those figured by the treasury stock method. Core et al. (2000) call this difference the "error in incremental shares." They use a relatively small sample (1,787 firm-years) over a short and recent time period, which gives them the detailed data to estimate Black-Scholes option values, and demonstrate that the market views these additional shares as dilutive. Thus Core et al. (2000) corroborate our inferences.

Our work differs from that of both Scott and Wier (2000) and Core et al. (2000) in that, rather than assessing the adequacy of the treasury stock method of dilution, we focus on how the market assesses dilution. Our results complement these other studies and make the following contributions to our understanding of the relations among earnings, valuation, and dilutive securities: First, we extend Ohlson's (1995) model and demonstrate analytically that rational investors should place less value on unexpected earnings when they anticipate dilution. Consistent with this hypothesis, our empirical tests show that the earnings response coefficient is a decreasing function of expected dilution, even when we measure earnings using existing accounting measures of diluted EPS. Second, we focus on the overall *level* of expected dilution, which provides a more comprehensive analysis than does Scott and Wier's (2000) focus on *changes* in measured accounting dilution. Third, there is no direct mathematical relation between our dependent variable (returns) and our dilution measure. Fourth, we use a large sample (63,656 firm-years) and an extensive set of control variables, so our results are more generalizable to the population of firms with outstanding dilutive securities. Finally, our results hold over regimes mandating different disclosure requirements for dilutive securities, thus providing evidence that the market attempted to assess dilution before today's more detailed accounting disclosures.

Our work also extends the earnings response coefficient (ERC) literature by documenting that dilutive securities significantly attenuate the relation between returns and earnings changes. This result is robust to the inclusion of control variables traditionally employed in the ERC literature, such as measures of size, risk, and growth (Collins and Kothari

1989), the absolute magnitude of unexpected earnings (Freeman and Tse 1992), and whether earnings are positive or negative (Hayn 1995).

Our results have implications both for future research and for the evolution of accounting standards. Our analysis indicates that dilutive securities attenuate the ERC even after we use diluted EPS in our regressions. This attenuation implies that SFAS No. 128 understates the total level of expected dilution, and that investors use additional information over that provided by GAAP-based EPS measures when valuing their share of future earnings. Consequently, both researchers and standard setters might consider alternative disclosures on dilutive securities to provide investors with more information to estimate their share of future cash flows.

The paper proceeds as follows. Section II presents both our model and hypotheses that explain the effect of expected dilution on the earnings/return relation. Section III describes the sample and research design, and Section IV presents our empirical results. Section V concludes the paper and discusses the implications of our results for financial reporting on dilutive securities.

II. HYPOTHESES DEVELOPMENT

Dilutive securities reduce the magnitude of abnormal stock returns associated with a given amount of unexpected earnings, because they reduce investors' expectations of their share of future firm cash flows. We explore how current shareholders incorporate unexpected earnings into share price when the firm has issued dilutive securities. We examine the effect of dilutive securities on the earnings/return relation in the Ohlson (1995) clean-surplus framework.[3] Following Ohlson (1995), we write the current share price as:

$$P_t = y_t + \sum_{\tau=1}^{\infty} \rho^{-\tau} E_t[\tilde{x}^a_{t+\tau}], \tag{1}$$

where:

P_t = market value or price per share of the firm's equity at date t;
y_t = the net book value per share at date t;
ρ = the required rate of return plus 1;
$E_t[\cdot]$ = the expected value operator conditioned on date t information;
x_t = earnings per share for the period (t − 1,t); and
$x^a_t = x_t - (\rho - 1)y_{t-1}$ = abnormal earnings.

Ohlson (1995) makes the following assumptions about the time-series behavior of abnormal earnings and other price-relevant information.

$$\tilde{x}^a_{t+1} = \omega x^a_t + \nu_t + \tilde{\varepsilon}_{1t+1} \tag{2}$$

$$\tilde{\nu}_{t+1} = \gamma \nu_t + \tilde{\varepsilon}_{2t+1} \tag{3}$$

---

[3] Modeling the ERCs of firms with and without dilutive securities in an option-pricing framework yields predictions identical to those obtained in the Ohlson (1995) framework. We present these results in the Appendix. We are grateful to Joseph Cherian for helpful discussions that assisted in the preparation of this appendix.

where:

ν_t = information other than abnormal earnings;
$\tilde{\varepsilon}_{1t+1}$ = unexpected abnormal earnings;
$\tilde{\varepsilon}_{2t+1}$ = unexpected non-earnings news;
ω = persistence parameter for unexpected earnings ($0 \leq \omega \leq 1$); and
γ = persistence parameter for non-earnings information ($0 \leq \gamma \leq 1$).

Adopting Ohlson's (1995) assumptions concerning the time-series behavior of abnormal earnings and other price-relevant information allows us to write unexpected returns as:

$$UR_t = \left(1 + \frac{\omega}{\rho - \omega}\right)\frac{\varepsilon_{1t}}{P_{t-1}} + \left(\frac{\rho}{(\rho - \omega)(\rho - \gamma)}\right)\frac{\varepsilon_{2t}}{P_{t-1}}, \tag{4}$$

where $1 + (\omega/(\rho - \omega))$ is the ERC. The ERC is increasing in the persistence of the earnings shock (ω) and decreasing in the required rate of return (ρ).[4] The ERC takes this form because the current earnings shock causes revisions in expectations of future earnings, and shareholders are discounting the stream $\{\omega^{\tau}\varepsilon_{1t}\}$ from ($\tau = 1,\infty$).

Since ε_{1t} is the unexpected abnormal EPS at time t, we can write it as the ratio of total unexpected abnormal earnings, UE_t, and the number of shares outstanding, N_t: $\varepsilon_{1t} = UE_t/N_t$. Discounting the stream $\{\omega^{\tau}\varepsilon_{1t}\}$ from ($\tau = 1,\infty$) is correct if the number of shares is expected to remain constant over time. Dilutive securities, however, make an increase in the number of shares likely. A dilutive increase in the shares outstanding in the future will reduce current shareholders' share of the revised stream of future earnings.[5]

When dilutive securities are outstanding, we define the revision in expected future earnings, arising from a time t earnings shock, that accrues to a current-period shareholder, as $\varepsilon_{1t,t+\tau} = \omega^{\tau}UE_t/E_t[N_{t+\tau}]$, where $E_t[N_{t+\tau}]$ is the dilution-adjusted number of shares expected to be outstanding in period $t + \tau$. A current shareholder's claim to the revision in the future earnings stream is less persistent when a firm has issued dilutive securities, since:

$$\varepsilon_{1t,t+\tau} = \frac{\omega^{\tau}UE_t}{E_t[N_{t+\tau}]} = \omega^{\tau}\varepsilon_{1t}\left(\frac{N_t}{N_t + E_t[\Delta N(t,t+\tau)]}\right) < \omega^{\tau}\varepsilon_{1t} \quad \text{from } (\tau = 1,\infty), \tag{5}$$

where $E_t[\Delta N(t,t + \tau)]$ is the expected number of dilutive shares issued between t and $t + \tau$.

Taking dilution into account, we can rewrite the ERC from equation (4) as:

$$\begin{aligned} ERC &= \left(1 + \sum_{\tau=1}^{\infty}\frac{\omega^{\tau}}{\rho^{\tau}}\left(\frac{N_t}{N_t + E_t[\Delta N(t,t+\tau)]}\right)\right) \leq \left(1 + \sum_{\tau=1}^{\infty}\frac{\omega^{\tau}}{\rho^{\tau}}\left(\frac{N_t}{N_t}\right)\right) \\ &= \left(1 + \frac{\omega}{\rho - \omega}\right). \end{aligned} \tag{6}$$

Equation (6) posits that the ERC for firm-years with dilutive securities will be smaller than the ERC for firm-years without dilutive securities, and that the difference between them will depend on the timing and magnitude of future dilutions, $E_t[\Delta N(t,t + \tau)]$. Accordingly, we test the hypothesis that the ERC is a decreasing function of expected dilution.

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[4] This is Ohlson's (1995) equation (6) from page 670, adjusting for risk as suggested on page 680.

[5] We are interested in dilutive shares. New shares sold at the market price, with proceeds invested in at least zero net present value projects, are not dilutive.

A simple example serves to illustrate. Assume a constant discount rate of 10 percent and a random walk process for earnings (i.e., $\omega = 1$). Further, assume that the current change in total earnings is \$100 and that there are 100 shares outstanding. If the firm has no dilutive securities, then each shareholder will expect to get \$1 in perpetuity, and the change in share price will be \$10. Adding this period's \$1 unexpected earnings yields an ERC of 11, as predicted.[6]

Now consider the case when a firm has dilutive securities outstanding, and current shareholders expect the firm to issue 25 dilutive shares five years from today. In this case, shareholders no longer have \$1 in perpetuity; instead, they have a \$1, four-year annuity and an eighty-cent perpetuity beginning in year five. The present value of this stream is:

$$PV = \$1 \times \left[\frac{1}{0.10}\left(1 - \frac{1}{(1.10)^4}\right)\right] + \left(\frac{\$0.80}{0.10}\right)\left(\frac{1}{(1.10)^4}\right) = \$8.63. \tag{7}$$

Adding 1 to this yields an ERC of 9.63, which is less than the predicted ERC of 11. The dilutive securities attenuate the effect of good news on common stock value, because some of the added value from the good news accrues to the holders of dilutive securities. Figure 1 illustrates how the effect of dilutive securities on the theoretical ERC varies with both the amount of potential dilution and the timing of dilution when $\omega = 1$, $\rho = 1.10$, and only one future dilutive event occurs at time t.

We cannot measure expected dilution directly, but we can develop a proxy for expected dilution based on the actual dilutive effect of exercise or conversion. The actual dilutive effect of exercise or conversion is given by:

$$\%\text{Dilution} = \left(\frac{P_t - X}{P_t}\right) \times \left(\frac{M}{N_t + M}\right), \tag{8}$$

where X is the strike price, P_t is the market price of one common share immediately preceding the time of exercise, M is the number of new shares issued, and N_t is the total shares outstanding prior to exercise.[7] We assume that holders of dilutive securities will not exercise if $P_t < X$ so %Dilution ≥ 0. %Dilution $\times$ $(N_t + M)$ is the number of shares issued in excess of those that the firm could purchase with the proceeds from exercise.

Since realized dilution is increasing in the number of dilutive securities when $P_t > X$, we argue that expected dilution is increasing in the number of dilutive securities (M). Expected dilution should also increase in the difference between the market value of the shares immediately preceding exercise and the option's strike price $(P_t - X)$. We therefore argue that expected dilution increases with good news about firm value. The following interrelated hypotheses consider how the existence and conversion probability of equity-like securities affect the theoretical ERC:

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[6] Dividend policy will affect expected dilution because only cash retained in the firm can be diluted. Higher dividend payout should lead to lower dilutive impact of reserved shares. We leave detailed consideration of this issue to future research.

[7] Convertible securities have embedded warrants with implicit strike prices equal to the market value of the convertible security divided by the number of common shares for which it is exchanged (see Ingersoll 1977, 310; Cox and Rubinstein 1985, 400).

TABLE 1
Common Shares Reserved for Conversion in Various Industries 1970–1995[a]

Industry Name	*Two-digit SIC Code*	*1970–79*	*1980–89*	*1990–95*
Agriculture, Forestry, and Fishing	01–09	7.78%	6.89%	9.93%
Mining and Construction	10–19	8.34%	9.33%	10.86%
Manufacturing	20–29	8.07%	8.16%	11.00%
	30–39	9.47%	10.61%	13.30%
Transportation, Communication, Utilities	40–49	5.43%	9.72%	12.16%
Wholesale and Retail Trade	50–59	8.15%	9.14%	12.41%
Finance, Insurance, and Real Estate	60–69	8.80%	9.08%	9.41%
Services	70–79	10.49%	11.81%	15.19%
	80–89	12.04%	12.37%	16.57%
Public Administration	90–99	12.61%	11.80%	12.15%
Total Sample		8.58%	9.79%	12.28%

[a] For each firm-year, we calculate PERCENT_{it} = Shares Reserved_{it}/(Shares Outstanding_{it} + Shares Reserved_{it}). Each cell shows the average PERCENT_{it} in each group of SIC codes for the time period shown.

the likelihood and magnitude of expected dilution, which in turn attenuates the effect of unexpected earnings on share price (i.e., ERCs). In other words, we expect investors to mark-to-market outstanding options.

Ideally, we want to measure how far "in the money" the options are expected to be at the exercise date. Firms experiencing increasing share prices are more likely to have options that are in the money. Since our research design relates current unexpected returns to current unexpected earnings, we cannot use current price change as an independent variable. Current measures of dilution from either treasury stock or option-pricing calculations are also problematic because each has a component related to current returns.[12] We take advantage of the path-dependent nature of conversion probabilities and use past price changes to separate firms based on the expected dilutive effect of reserved shares. Using all sample firm-years, we examine the distribution of annual price changes in the year prior to measuring earnings changes and returns. We then identify those firms whose stock price increases were in the top quartile of this distribution, and assign to the dummy variable HIRET the value of 1 for these firms and 0 otherwise. Similarly, we create a LOWRET dummy variable for firms in the bottom quartile of the price change distribution. These return-based proxies capture news from $t-2$ to $t-1$. Our second proxy for news is based on earnings. We assign to the dummy variable POSCHANGE a value of 1 if the change in earnings from period $t-1$ to period t is positive, and 0 otherwise.[13]

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[12] This is true for option-based adjustments as well as treasury stock-based adjustments because option values are increasing in market price. See, for example, Stoll and Whaley (1993, 220).

[13] The variable POSCHANGE also serves as a control because we cannot assume a symmetric market response to good and bad earnings news (Basu 1997). We are grateful to an anonymous referee for suggesting this specification.

Control Variables

Collins and Kothari (1989) demonstrate that the ERC will vary with firm size, depending on the window over which the returns are measured. They also demonstrate that the ERC is positively related to growth opportunities and inversely related to risk. Firms that issue dilutive securities are smaller and riskier, and have greater growth opportunities than other firms (Mikkelson 1981; Brennan and Schwartz 1988; Smith and Watts 1992; Stein 1992). Our tests control for these factors to demonstrate that dilutive securities play a significant incremental role in moderating the earnings/return relation.

We use the log of the opening market value of equity as our size proxy (LMVALUE).[14] The variance of the firm's monthly returns calculated over the prior three years (RETVAR) is our risk proxy. We employ two proxies for growth opportunities. First, the ratio of a firm's market value to its book value measured at the beginning of the year (MKTTOBK) is a widely used growth proxy that incorporates the market value of intangible assets beyond that reported in the firm's accounting numbers. Second, Mikkelson (1981) and Smith and Watts (1992) document a positive association between the use of dilutive securities and growth. If firms that issue dilutive securities have higher growth prospects, all else being equal, they will have higher ERCs. This effect could dominate any direct negative effect of the dilutive shares. We control for differences between firms that use and do not use dilutive securities with an indicator variable (DS_USER). This variable equals 1 in firm-years with dilutive securities and 0 otherwise.

Freeman and Tse (1992) show that marginal price responses to unexpected earnings are a declining function of the magnitude of the earnings surprise. Risky firms with growth opportunities may well have large earnings surprises. Since firms with these characteristics are likely to use dilutive securities (Brennan and Schwartz 1988; Stein 1992), we control for the absolute value of the earnings surprise (|DIFFINC|)to ensure that the coefficient on the percent of reserved shares is not simply an artifact of the nonlinearity documented by Freeman and Tse (1992).

Hayn (1995) demonstrates that the market's response to earnings changes is diminished when earnings are negative (i.e., when the firm experiences a loss). She attributes the attenuation to shareholders' option to liquidate the firm if the liquidation value exceeds the present value of the firm's future cash flows. Firms that report losses may be more likely to issue contingent claims in lieu of either cash compensation (executive stock options) or straight debt (convertible debt). To ensure that the reserved-shares variable is not simply picking up the effect of negative reported earnings, we follow Hayn (1995) and control for reported losses by defining POSEARN as equal to 1 if annual total income is greater than 0 in both the current and the prior fiscal year, and 0 otherwise.

Model Specification

We model the ERC as a function of the percentage of shares reserved for conversion, size, risk, growth, the magnitude of the earnings surprise, and the presence of positive earnings, in order to test H1a. We use the following regression model to test H1a, which predicts $\beta_1 < 0$:

---

[14] Results are robust to use of the log of opening total assets as the size proxy.

$$
\begin{aligned}
UR12_{it} &= \alpha + \beta\ DIFFINC_{it} + e_{it};\\
\beta &= \beta_0 + \beta_1 PERCENT_{it} + \beta_2 LMVALUE_{it} + \beta_3 RETVAR_{it}\\
&\quad + \beta_4 MKTTOBK_{it} + \beta_5 DS\_USER_{it} + \beta_6 |DIFFINC_{it}|\\
&\quad + \beta_7 POSEARN_{it}. \qquad \text{(Model 1)}
\end{aligned}
$$

$UR12_{it}$ is the size-adjusted return for firm i, cumulated over a 12-month period ending with the last day of fiscal year t. We use the first difference in annual total income before extraordinary items as our proxy for unexpected abnormal earnings, and scale by the opening market value of equity to create $DIFFINC_{it}$.[15] All other variables are as described above.

To test H1b, we augment Model 1 so that the influence of PERCENT is allowed to vary with both price-based and earnings-based proxies for conversion likelihood. The model containing the price-based proxies follows:

$$
\begin{aligned}
UR12_{it} &= \alpha + \beta\ DIFFINC_{it} + e_{it};\\
\alpha &= \alpha_0 + \alpha_2 HIRET_{it} + \alpha_3 LOWRET_{it};\\
\beta &= \beta_0 + \beta_1 PERCENT_{it} + \beta_2 LMVALUE_{it} + \beta_3 RETVAR_{it}\\
&\quad + \beta_4 MKTTOBK_{it} + \beta_5 DS\_USER_{it} + \beta_6 |DIFFINC_{it}|\\
&\quad + \beta_7 POSEARN_{it} + \beta_8 HIRET_{it} + \beta_9 LOWRET_{it};\\
\beta_1 &= \gamma_0 + \gamma_1 HIRET_{it} + \gamma_2 LOWRET_{it}. \qquad \text{(Model 2)}
\end{aligned}
$$

Our particular interest is in the three-way interactions of DIFFINC and PERCENT with HIRET and LOWRET.[16] We expect to see a negative coefficient on the interaction of DIFFINC, PERCENT, and HIRET (i.e., $\gamma_1 < 0$) and a positive coefficient on the interaction of DIFFINC, PERCENT, and LOWRET (i.e., $\gamma_2 > 0$). A negative γ_1 will indicate that as the likelihood and dilutive effect of conversion (as proxied by the stock price run-up over the prior year) increase, the current stockholders' share of the revised expectation of future earnings becomes smaller. A positive γ_2 indicates that as the likelihood and dilutive effect of conversion decrease, the current stockholders' share of the revised expectation of future earnings becomes larger.[17]

Alternately, we use our earnings-based definition of news in Model 2, and replace the variables HIRET and LOWRET with the variable POSCHANGE. We predict a negative coefficient on the interaction of DIFFINC, PERCENT, and POSCHANGE. Finally, we combine both the price-based and the earnings-based news proxies in the same regression model. In this specification, the stock price run-up captures news about the firm up to period $t-1$, while the contemporaneous earnings change captures news about the firm from period $t-1$ to period t.

Table 2 presents descriptive statistics for our sample firms. Panel A presents distributional characteristics, and Panel B presents both Pearson and Spearman correlations for the variables used in the analysis. Comparison of the mean and median values in Panel A

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[15] We discuss the limitations of this proxy in Section IV.

[16] Jegadeesh and Titman (1993) and Soffer and Walther (2000) document persistence in stock returns measured over intervals of less than one year. We therefore employ HIRET and LOWRET as main effects to control for the correlation between past and current returns.

[17] Using stock price changes measured over the four years preceding the current fiscal year does not change our results.

TABLE 2
Descriptive Statistics for 63,656 Firm-Year Observations from 1970–1995

Panel A: Distributional Characteristics of Independent Variables

	Mean	*Median*	*Standard Deviation*
DIFFINC	0.0107	0.0106	0.1266
PERCENT	12.76%	7.92%	14.88%
MVALUE	561.222	55.389	2698.581
RETVAR	0.6287	0.4732	0.5236
MKTTOBK	1.3951	1.0856	0.9460
DS_USER	0.8764	1.0000	0.3291
|DIFFINC|	0.0739	0.0340	0.1033
POSEARN	0.7596	1.0000	0.4273

DIFFINC = the first difference in annual total income scaled by MVALUE;
PERCENT = a firm's shares reserved for conversion as a percentage of total shares outstanding plus shares reserved for conversion;
MVALUE = the market value of a firm's equity measured at the beginning of the fiscal year ($MM);
RETVAR = the monthly variance of a firm's raw returns measured over the three-year period prior to the year total income is measured;
MKTTOBK = the ratio of a firm's market value to its book value measured at the beginning of the fiscal year;
DS_USER = an indicator variable taking on a value of 1 if the firm has shares reserved for conversion or the exercise of options or warrants, and 0 otherwise; and
POSEARN = an indicator variable taking on a value of 1 if annual total income is greater than zero in both the current and the prior fiscal year, and 0 otherwise.

(*Continued on next page*)

TABLE 2 (Continued)

Panel B: Correlation coefficients between variables used in the analysis of the effect of dilutive shares on the earnings/return relation. P-values are in parentheses. Pearson correlations are above the diagonal, and Spearman correlations are below the diagonal.

	UR12	*DIFFINC*	*PERCENT*	*LMVALUE*	*RETVAR*	*MKTTOBK*	*DS_USER*	*|DIFFINC|*	*POSEARN*
UR12		0.297 (0.0001)	−0.034 (0.0001)	0.041 (0.0001)	−0.090 (0.0001)	0.172 (0.0001)	−0.010 (0.011)	−0.024 (0.0001)	0.207 (0.0001)
DIFFINC	0.398 (0.0001)		0.006 (0.148)	−0.055 (0.0001)	0.032 (0.0001)	0.005 (0.180)	−0.002 (0.699)	0.135 (0.0001)	0.065 (0.0001)
PERCENT	−0.408 (0.0001)	0.013 (0.002)		−0.108 (0.0001)	0.218 (0.0001)	0.005 (0.171)	0.322 (0.0001)	0.144 (0.0001)	−0.197 (0.0001)
LMVALUE	0.080 (0.0001)	−0.062 (0.0001)	−0.063 (0.0001)		−0.432 (0.0001)	0.208 (0.0001)	0.084 (0.0001)	−0.329 (0.0001)	0.263 (0.0001)
RETVAR	−0.130 (0.0001)	0.028 (0.0001)	0.274 (0.0001)	−0.486 (0.0001)		0.140 (0.0001)	0.121 (0.0001)	0.213 (0.0001)	−0.301 (0.0001)
MKTTOBK	0.225 (0.0001)	0.040 (0.0001)	0.039 (0.0001)	0.371 (0.0001)	0.026 (0.0001)		0.043 (0.0001)	−0.164 (0.0001)	−0.038 (0.0001)
DS_USER	−0.022 (0.0001)	0.008 (0.034)	0.570 (0.0001)	0.080 (0.0001)	0.163 (0.0001)	0.100 (0.0001)		0.032 (0.0001)	−0.067 (0.0001)
|DIFFINC|	0.021 (0.0001)	0.154 (0.0001)	0.146 (0.0001)	−0.389 (0.0001)	0.280 (0.0001)	−0.308 (0.0001)	0.039 (0.0001)		−0.530 (0.0001)
POSEARN	0.234 (0.0001)	0.095 (0.0001)	−0.190 (0.0001)	−0.266 (0.0001)	−0.303 (0.0001)	0.060 (0.0001)	−0.067 (0.0001)	−0.488 (0.0001)	

UR12 = the 12-month raw return less the 12-month return on the corresponding size portfolio; and
LMVALUE = the log market value of a firm's equity measured at the beginning of the fiscal year.
Other variables are as defined in Panel A.

reveals that many of the variables are skewed, so we winsorize all variables at 1 percent and 99 percent. (Subsequently reported sensitivity analysis uses rank regressions to ensure that our results are not attributable to extreme values.) Not surprisingly, Panel B reveals a strong positive correlation between PERCENT and DS_USER. The correlations in Panel B also show that, consistent with our earlier discussion, firms that use dilutive securities more extensively are likely to be riskier, with more variable earnings.

IV. RESULTS AND DISCUSSION

This section presents the results of the empirical tests of H1a and H1b. Table 3 presents our test of H1a, which predicts that the ERC is a decreasing function of the number of dilutive securities. We provide additional analysis that demonstrates the robustness of the results to different specifications. Table 4 presents our test of H1b, which predicts that good news about firm value increases the negative effect of reserved shares on ERCs. All reported test statistics use White (1980) heteroskedasticity-consistent standard errors, and reported significance levels are for two-tailed tests.

The Effect of Dilutive Securities on the ERC: H1a

We begin with a simple model that regresses $UR12_{it}$ on only DIFFINC and DIFFINC × PERCENT. The estimated coefficient on the interaction of PERCENT with DIFFINC is negative (−0.883) and significant (t-statistic = −8.56), supporting H1a. We then estimate Model 1 to control for other factors that affect the earnings/return relation. Table 3 presents the results, pooled both over all years and by decade. Both the pooled and the per-decade results support H1a. Using all sample years, the estimated coefficient on the interaction of PERCENT with DIFFINC is −0.611 (t-statistic = −4.29).[18] The typical firm (i.e., a firm with dilutive securities, positive earnings, and average values of lagged market value, return variance, market-to-book ratio, and absolute value of the earnings shock) would have an estimated ERC of 1.89 if it had reserved shares equal to 2 percent of its current shares outstanding. This same firm with reserved shares equal to 20 percent of its current shares outstanding would have an estimated ERC of 1.80. Moving from a low percentage of reserved shares to a high percentage of reserved shares reduces the ERC by 4.76 percent.

The remaining columns in Table 3 estimate the relation over the 1970s, 1980s, and 1990s. The negative impact of reserved shares on the ERC (the interaction of PERCENT and DIFFINC) is significant in each decade. The coefficient estimates (t-statistics) for the 1970s, 1980s, and 1990s are −0.774 (−2.98), −0.532 (−2.47), and −0.690 (−2.49), respectively.

The strength of the results is comparable across the decades despite the fact that the level and precision of disclosures pertaining to executive stock options increased after the political debate concerning executive compensation commencing in 1991 (Bloomenthal and Holme Roberts & Owen 1998). The SEC adopted new, more rigorous disclosure requirements in 1992 and 1993 (e.g., Securities Act Releases 6962 and 7032). The FASB (1995) also began deliberations on accounting for stock options, culminating in SFAS No. 123,

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[18] Collinearity diagnostics (Jobson 1991; Belsley et al. 1980) indicate that only one eigenvector has a condition index (13.8) in excess of 10. Further, the variance inflation factor (VIF) on DIFFINC × PERCENT is the second smallest of the interactions. If we remove the interaction with the highest VIF (DIFFINC × DS_USER), the sign and significance of DIFFINC × PERCENT remains unchanged. These diagnostics indicate that multicollinearity is unlikely to affect our inferences. In addition, the Table 3 results are robust to the following specifications: (1) inclusion of main effects for all of the interactive variables, (2) employing both the prior earnings levels and the current earnings change (Easton and Harris 1991; Ali and Zarowin 1992; Biddle et al. 1995), and (3) mean-adjusting all the independent variables prior to calculating interactions (Aiken and West 1991).

TABLE 3
Regression Analysis of the Effect of Dilutive Shares on the Earnings/Return Relation

$$UR12_{it} = \alpha + \beta\, DIFFINC_{it} + e_{it}$$

$$\beta = \beta_0 + \beta_1 PERCENT_{it} + \beta_2 LMVALUE_{it} + \beta_3 RETVAR_{it} + \beta_4 MKTTOBK_{it} + \beta_5 DS\_USER_{it} + \beta_6 |DIFFINC_{it}| + \beta_7 POSEARN_{it}$$

		All Years	*1970–79*	*1980–89*	*1990–95*
Intercept		−0.042	−0.062	−0.026	−0.046
		(−28.85)	(−23.15)	(−11.41)	(−15.07)
DIFFINC	(+)	0.879	0.463	0.895	0.907
		(12.19)	(2.01)	(8.24)	(6.24)
DIFFINC × PERCENT	(−)	−0.611	−0.774	−0.532	−0.690
		(−4.29)	(−2.98)	(−2.47)	(−2.49)
DIFFINC × LMVALUE	(+)	0.070	0.088	0.065	0.054
		(7.90)	(4.23)	(5.01)	(3.17)
DIFFINC × RETVAR	(−)	−0.055	−0.124	−0.035	−0.030
		(−1.88)	(−2.34)	(−0.78)	(−0.55)
DIFFINC × MKTTOBK	(+)	0.271	0.839	0.266	0.169
		(7.76)	(3.25)	(4.52)	(3.67)
DIFFINC × DS_USER	(+)	0.277	0.168	0.249	0.494
		(5.49)	(1.93)	(3.28)	(4.61)
DIFFINC × |DIFFINC|	(−)	−0.238	−2.076	−2.427	−2.571
		(−25.75)	(−17.29)	(−17.29)	(−12.94)
DIFFINC × POSEARN	(+)	0.127	1.018	1.172	1.461
		(29.91)	(18.28)	(18.35)	(12.35)
Adj. R^2		15.57%	19.46%	14.86%	14.05%
n		63,656	20,386	26,207	17,063

The dependent variable is UR12 (the 12-month raw return less the 12-month return on the corresponding size portfolio).
t-statistics calculated with heteroskedasticity-consistent variance estimators as in White (1980) in parentheses.

DIFFINC = the first difference in annual total income scaled by MVALUE, the opening market value of equity;
PERCENT = a firm's shares reserved for conversion as a percentage of total shares outstanding plus shares reserved for conversion;
LMVALUE = the log market value of a firm's equity measured at the beginning of the fiscal year;
RETVAR = the variance of a firm's raw returns measured over the three-year period prior to the measurement of UR12;
MKTTOBK = the ratio of a firm's market value to its book value measured at the beginning of the fiscal year;
DS_USER = an indicator variable taking on a value of 1 if the firm has shares reserved for conversion or the exercise of options or warrants, and 0 otherwise; and
POSEARN = an indicator variable taking on a value of 1 if annual total income is greater than 0 in both the current and the prior fiscal year, and 0 otherwise.

Accounting for Stock-Based Compensation," in October 1995. Table 3 suggests that even during earlier periods, when the accounting disclosures were less extensive and detailed than they are now, the market impounded expectations of future dilution into stock price.

The results in Table 3 are based on changes in total earnings, consistent with equation (6), which demonstrates how dilutive securities attenuate the ERC. Since accounting standards attempt to incorporate earnings dilution in the EPS calculation, we examine whether the GAAP adjustments for dilution are sufficient to remove the negative effect of our dilution proxy, PERCENT, on the ERC. We obtain comparable results when we replicate H1a replacing DIFFINC, the scaled year-to-year change in total income before extraordinary items, with (1) the scaled year-to-year change in primary EPS before extraordinary items (Compustat data item #58), and (2) the scaled year-to-year change in fully diluted EPS before extraordinary items (Compustat data item #57). The coefficient (t-statistic) on the interaction of PERCENT with the change in primary EPS is −0.609 (−4.06), while the coefficient (t-statistic) on the interaction of PERCENT with the change in fully diluted EPS is −0.631 (−3.61).[19] Thus, the negative sign on the interaction of PERCENT with each of the EPS measures, each of which contains a GAAP-based measure of dilution, suggests that the GAAP dilution adjustments are less than those made by the market.

It is likely that GAAP dilution adjustments are inadequate because of the manner in which the treasury stock method incorporates outstanding options. The treasury stock method treats all options as if today were the exercise date, and does not attempt to assess dilution that may arise from future price movements. For options at or out of the money, the treasury stock method reports no dilution. Since executive stock options are generally issued at the money, GAAP ignores the expected dilution attributable to many outstanding options. Core et al. (2000) provide evidence that the treasury stock method understates expected dilution. However, an alternative explanation for the negative sign on the interaction even when we use the diluted EPS numbers is that our tests fail to control for some omitted correlated factor.

Robustness Checks

We run several additional tests (untabulated) to rule out the possibility that our results are driven by a correlated omitted factor. These tests include using an "overly diluted" EPS measure, estimating regressions using only issuers of dilutive securities, and estimating industry and firm-specific regressions. In addition, we employ rank regressions to control for the skewness in our data.

We rerun the Table 3 regressions using an overly diluted EPS as our measure of DIFFINC, where we compute EPS as earnings available to common shareholders divided by shares outstanding plus total shares reserved for conversion. Core et al. (2000) suggest that this overly diluted EPS measure is a reliable and conservative estimate of EPS. Since this earnings measure overstates dilution, the implication of current unexpected earnings for future earnings should *increase* with the number of shares reserved. This is opposite from the prediction for the original definition of DIFFINC, so that for this specification the interaction of DIFFINC and PERCENT should be positive rather than negative. Conversely, if PERCENT is simply capturing a correlated omitted factor, the coefficient on the interaction of PERCENT and overly diluted EPS should remain negative. Consistent with the contention that PERCENT proxies for expected dilution and that the treasury stock method understates dilution, the estimated coefficient on the interaction of PERCENT and the change in overly diluted EPS is positive (0.490, t-statistic = 3.81).

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[19] The similarity of these results is consistent with the high correlation between scaled changes in total earnings and scaled changes in primary EPS (fully diluted EPS) of 0.98 (0.87).

As additional corroboration that PERCENT is not capturing some correlated omitted factor specific to firms or industries that issue dilutive securities, we estimate regressions on the subset of firms that issue dilutive securities, industry regressions, and firm-specific regressions. We first re-estimate the Table 3 regressions on only those observations for which DS_USER = 1. Consistent with our prior results, the interaction of PERCENT with DIFFINC is significantly negative in this specification, with a t-statistic (significance level) of −4.10 (0.0001).[20]

We next estimate industry- and firm-specific regressions to provide additional assurance that our results are not driven by differences between issuers and nonissuers of dilutive securities. These tests also help address concerns that our proxy for unexpected earnings—the first difference in annual earnings—does not allow for cross-sectional variation in the persistence of earnings shocks (ω) or the cost of capital (ρ), nor does it consider the nonearnings-related information that may enter into earnings expectations (ν_t). We expect the persistence of shocks and the cost of capital to be relatively constant both within industries and for individual firms. The industry regressions also control for any industry-wide, nonearnings-related information. If nonearnings-related information is random across firms, then it is not a concern for our tests.

We estimate the industry regressions separately for the 100 three-digit SIC codes that have at least 200 firm-years of data between 1970 and 1995. To control for variation between firms within an industry, we use all of the control variables reported in the Table 3 regressions. We then aggregate the 100 industry-specific t-statistics on the interaction between DIFFINC and PERCENT to form a Z-statistic.[21] Results of the industry regressions corroborate the results presented in Table 3, showing that they are not attributable to characteristics that differ across industries. The mean (median) coefficient on the interaction of DIFFINC and PERCENT is −1.089 (−1.236). The Z-statistic on the mean coefficient is −6.30 (p-value < 0.001). Sixty-four percent of the industry regressions produce a negative coefficient on the interaction of DIFFINC and PERCENT.

We estimate 319 firm-specific regressions for firms that meet the following criteria: We select all firms with at least 20 years of data between 1970 and 1995 and separate them into quintiles based on the time-series variability of PERCENT. We select firms in the top two quintiles to ensure sufficient variability in the PERCENT variable. Since each firm is its own control, we estimate a standard regression of returns on earnings changes, including the interaction on PERCENT and DIFFINC. We include |DIFFINC| as a control variable since the magnitude of a firm's earnings shocks can vary over time.

The mean (median) coefficient on the interaction of DIFFINC and PERCENT is −2.634 (−1.256) with a p-value of 0.04 (0.06). The range from the 1st to 3rd quartiles of the firm-specific regression coefficients is from −12.04 to 6.79 and 55 percent of the firm-specific regressions produce a negative coefficient on the interaction of DIFFINC and PERCENT.

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[20] This specification also rules out the possibility that a spurious relation induced by including both DS_USER and PERCENT in the same regression model drives the main Table 3 results.

[21] We calculate the Z-statistic by aggregating the t-statistics from the industry regressions as follows: $Z_i = (1/\sqrt{N_i})\,\Sigma_{j=1}^{N_i}\, t_j/\sqrt{k_j/(k_j-2)}$, where t_j = t-statistic for firm j, k_j = degrees of freedom in the regression for firm j, and N_i = number of firms in sample i. See, for example, Christie (1990), Barth (1994), and Dechow et al. (1994).

TABLE 4
Regression Analysis of the Effect of Dilutive Shares on the Earnings/Return Relation after Controlling for the Likelihood and Magnitude of Expected Dilution

$$UR12_{it} = \alpha + \beta\, DIFFINC_{it} + e_{it}$$

$$\alpha = \alpha_0 + \alpha_2 HIRET_{it} + \alpha_3 LOWRET_{it}$$

$$\begin{aligned}\beta = \beta_0 &+ \beta_1 PERCENT_{it} + \beta_2 LMVALUE_{it} + \beta_3 RETVAR_{it}\\ &+ \beta_4 MKTTOBK_{it} + \beta_5 DS\_USER_{it} + \beta_6 |DIFFINC_{it}|\\ &+ \beta_7 POSEARN_{it} + \beta_8 HIRET_{it} + \beta_9 LOWRET_{it} + \beta_{10} POSCHANGE_{it}\end{aligned}$$

$$\beta_1 = \begin{cases} \gamma_0 + \gamma_1 HIRET_{it} + \gamma_2 LOWRET_{it} & MODEL\ 2(a)\\ \gamma_0 + \gamma_3 POSCHANGE_{it} & MODEL\ 2(b)\\ \gamma_0 + \gamma_1 HIRET_{it} + \gamma_2 LOWRET_{it} + \gamma_3 POSCHANGE_{it} & MODEL\ 2(c) \end{cases}$$

		MODEL 1	*MODEL 2(a)*	*MODEL 2(b)*	*MODEL 2(c)*
Intercept		−0.042 (−28.85)	−0.039 (−20.45)	−0.033 (−19.55)	−0.033 (−16.15)
DIFFINC	(+)	0.879 (12.19)	0.913 (12.20)	0.866 (11.74)	0.894 (11.74)
DIFFINC × PERCENT	(−)	−0.611 (−4.29)	−0.562 (−2.70)	−0.045 (−0.24)	−0.041 (−0.17)
DIFFINC × LMVALUE	(+)	0.070 (7.90)	0.061 (6.87)	0.066 (7.51)	0.058 (6.55)
DIFFINC × RETVAR	(−)	−0.055 (−1.88)	−0.085 (−2.84)	−0.049 (−1.69)	−0.079 (−2.66)
DIFFINC × MKTTOBK	(+)	0.271 (7.76)	0.276 (7.90)	0.277 (7.80)	0.280 (7.89)
DIFFINC × DS_USER	(+)	0.277 (5.49)	0.292 (5.80)	0.280 (5.56)	0.296 (5.86)
DIFFINC × |DIFFINC|	(−)	−0.238 (−25.75)	−2.299 (−24.92)	−2.183 (−22.42)	−2.147 (−22.10)
DIFFINC × POSEARN	(+)	0.127 (29.91)	1.073 (28.28)	1.173 (29.51)	1.111 (27.78)
HIRET	(+)		0.023 (6.34)		0.023 (6.40)
LOWRET	(−)		−0.035 (−9.93)		−0.031 (−8.73)
DIFFINC × HIRET			0.189 (3.14)		0.184 (3.05)
DIFFINC × LOWRET			−0.153 (−3.18)		−0.158 (−3.28)

(*Continued on next page*)

TABLE 4 (Continued)

		MODEL 1	*MODEL 2(a)*	*MODEL 2(b)*	*MODEL 2(c)*
DIFFINC × PERCENT × HIRET	(−)		−0.609 (−1.70)		−0.604 (−1.68)
DIFFINC × PERCENT × LOWRET	(+)		0.266 (0.95)		0.293 (1.04)
DIFFINC × POSCHANGE				−0.131 (−2.64)	−0.085 (−1.72)
DIFFINC × PERCENT × POSCHANGE	(−)			−0.994 (−3.93)	−0.938 (−3.74)
Adj. R^2		15.57%	15.95%	15.71%	16.04%
n		63,656	63,656	63,656	63,656

The dependent variable is UR12 (the 12-month raw return less the 12-month return on the corresponding size portfolio).
t-statistics calculated with heteroskedasticity-consistent variance estimators as in White (1980) in parentheses.

DIFFINC = the first difference in annual total income scaled by MVALUE, the opening market value of equity;
PERCENT = a firm's shares reserved for conversion as a percentage of total shares outstanding plus shares reserved for conversion;
LMVALUE = the log market value of a firm's equity measured at the beginning of the fiscal year;
RETVAR = the variance of a firm's raw returns measured over the three-year period prior to the measurement of UR12;
MKTTOBK = the ratio of a firm's market value to its book value measured at the beginning of the fiscal year;
DS_USER = an indicator variable taking on a value of 1 if the firm has shares reserved for conversion or the exercise of options or warrants, and 0 otherwise;
POSEARN = an indicator variable taking on a value of 1 if annual total income is greater than 0 in both the current and the prior fiscal year, and 0 otherwise;
HIRET = a value of 1 if the firm's stock price change in year t − 1 is in the top quartile of year t − 1 price changes and 0 otherwise;
LOWRET = a value of 1 if the firm's stock price change in year t − 1 is in the bottom quartile of year t − 1 price changes and 0 otherwise; and
POSCHANGE = an indicator variable taking on a value of 1 if DIFFINC is positive, and 0 otherwise.

The Z-statistic on the mean coefficient is −3.49, with a p-value of 0.001.[22] These results are somewhat weaker than the industry regressions. A potential explanation is that the firm-specific regressions use 7,298 firm-years or 11.5 percent of the total sample, whereas the industry regressions use 48,679 firm-years or 76.5 percent of the sample.

As a final specification check, we perform rank regressions following the methodology of Cheng et al. (1992) because our data are skewed. We rank every continuous variable and scale the ranks to lie between 0 and 1. Results are robust to this specification. The interaction of PERCENT with DIFFINC in the rank regression produces a coefficient estimate (t-statistic) of −0.25 (−3.51).

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[22] The firm-specific analysis has the following additional benefits: (1) results are not subject to problems of contemporaneous correlation of errors, and (2) multicollinearity is not an issue because of the simple model employed.

These additional tests suggest that the results from Table 3 are not attributable to PERCENT's capturing a correlated omitted factor. A regression of abnormal returns on an overly diluted measure of EPS produces a positive interaction with PERCENT, while the interaction between DIFFINC and PERCENT continues to be negative and significant in regressions that include only firms with outstanding dilutive securities, regressions by industry, and firm-specific regressions. Additionally, results are robust to the rank regression specification.

Is the Dilutive Effect Stronger in the Presence of Good News: H1b

Table 4 presents four regression models. Model 1 presents our test of H1a (Column 1 from Table 3), and Models 2a, 2b, and 2c present alternative tests of H1b. We use all firm-years because Table 3 does not reveal temporal shifts in the relation between earnings changes and returns.

Models 2a, 2b, and 2c extend Model 1 to include price- and earnings-based proxies for the likelihood and dilutive impact of conversion. Model 2a incorporates the price-based proxies HIRET and LOWRET. The HIRET (LOWRET) firms are those for whom stock price changes over the prior year were in the top (bottom) quartile of all sample firms. We focus on the interaction of HIRET (LOWRET) with income changes (DIFFINC) and the percentage of shares reserved for conversion/exercise (PERCENT), since our theory predicts that the dilutive effect of reserved shares will be larger (smaller) for firms with rising (falling) prices.

The coefficient estimate for the three-way interaction of DIFFINC, PERCENT, and HIRET is negative (−0.609) and marginally significant with a p-value of 0.10 (t-statistic = −1.70). This provides weak evidence that, among firms with dilutive securities, the market expects current investors' share of future earnings to decline as the likelihood of conversion/exercise increases. This negative relation reflects the higher expected dilution for firms with price increases, and is consistent with investors' implicitly marking outstanding options and convertible securities to market. The three-way interaction of DIFFINC, PERCENT, and LOWRET is insignificant (t-statistic = 0.95). The insignificantly positive coefficient suggests that increases and decreases in firm value do not have symmetrical effects.[23] The two-way interaction of DIFFINC and PERCENT remains negative (−0.562) and significant (t-statistic = −2.70) in Model 2a despite the presence of *HIRET*. These results imply that the market perceives dilutive securities as costly in general, while anticipating more dilution for firms that have had larger stock price run-ups.

Model 2b uses the earnings-based conversion proxy POSCHANGE. We are interested in the interaction of DIFFINC, PERCENT, and POSCHANGE. The coefficient on this interaction is −0.994 (t-statistic = −3.93). This result suggests that the market's reaction to good earnings news is attenuated by the likelihood that some of the value added by this news accrues to the option holders. The coefficient estimate on the interaction between DIFFINC and PERCENT captures the anticipated dilution of negative and zero earnings changes. This interaction is insignificant, consistent with current shareholders expecting to share positive earnings changes, but not expecting to share negative earnings changes, with the holders of dilutive securities.

Model 2c incorporates both the price-based and the earnings-based proxies for the spread between strike and market prices in the same regression model. The price-based proxies incorporate news about the firm up to time t − 1, and the earnings-based proxy picks up news about the firm from time t −1 to time t. The coefficients on both proxies for positive news, the interaction of DIFFINC, PERCENT, and HIRET, and the interaction of DIFFINC, PERCENT, and POSCHANGE, maintain their statistically significant negative signs, consistent with the hypothesis that equity investors view their share of unexpected earnings as being less permanent as the likelihood of sharing it with other stakeholders' increases.

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[23] This is perhaps related to the fact that option value increases in the value of the underlying faster than it decreases in the value of the underlying (i.e., options are convex). This presents an interesting question for future research.

V. CONCLUSIONS

Firms are increasingly using stock options and convertible securities. The FASB has expressed concern that investors do not fully understand the value implications of these securities. Adapting Ohlson's (1995) model, we demonstrate analytically that rational investors should place less value on unexpected earnings when they expect dilutive securities to lead to future earnings dilution.

We use the percentage of shares held in reserve for conversion of dilutive securities as a proxy for expected dilution, and show empirically that the ERC is a decreasing function of expected dilution. This relation holds after controlling for firm size, risk, growth opportunities, the magnitude of the unexpected earnings, and the sign of current earnings. Further, we show that the effect of reserved shares is greater for firms when the likelihood and dilutive effects of conversion are greater (i.e., firms that have recently experienced "good news").

A limitation of our study is that we employ proxies rather than exact measures for both unexpected abnormal earnings and expected dilution. Our proxy for unexpected abnormal earnings, the first difference in annual earnings, does not allow for cross-sectional variation in persistence, nonearnings information, or the cost of capital. If the error in our proxy is correlated with reserved shares, then our results may be affected by correlated omitted factors. We address this issue, in part, through our control variables and industry- and firm-specific regressions. We base our proxy for expected dilution on the total number of shares reserved for conversion or exercise. Because we cannot quantify expected dilution precisely, we do not prescribe how accountants should measure the earnings of firms that issue dilutive securities.

Our results contribute to the ERC literature by identifying expected earnings dilution as an additional firm characteristic that provides investors with information about their share of future firm cash flows. More broadly, these results illustrate a cost of issuing dilutive securities. The benefits of issuing convertibles include lower interest for convertible debt and higher prices for convertible preferred, whereas the issuance of options provides compensation that both conserves cash and aligns the interests of management and employees with those of shareholders. However, an important cost associated with these securities is that the market will discount favorable earnings performance when future dilution of these earnings is likely.

Recent changes in disclosures pertaining to dilutive securities do not fully reflect the effect of these securities on firm value. The fact that reserved shares further attenuate the ERC even after we employ diluted EPS in our regression analysis implies that SFAS No. 128 understates expected dilution. Additionally, while SFAS No. 123 requires firms to disclose the market value of options on the grant date, it does not require firms to disclose subsequent changes in that value. Our results are consistent with investors' attempting to mark dilutive securities to market, and hence, we would expect investors to use more timely, marked-to-market option values if they were available.

A useful avenue for future research would be to explore how best to communicate to investors information concerning dilutive securities. Alternatives include simple disclosure of the marked-to-market values, EPS adjustments similar to those in Core et al. (2000), and inclusion of the changes in the marked-to-market values in reported income.

APPENDIX

Another way to think about the dilutive effects of option-like securities is to approach the problem with option-pricing tools. If we consider an all-equity firm with only common equity outstanding, then the value of the common equity is equal to firm value:

$$E = V \tag{I}$$

If we consider an all-equity firm that has issued executive stock options (ESOs), then the value of the common equity is the value of the firm less the value of the ESOs:[24]

$$E = V - nC(V, X, T, r, \sigma) \tag{II}$$

where:

n = number of ESOs;
C = value of an ESO;
E = value of common equity;
V = value of the firm;
X = strike price;
T = time to expiration;
r = risk-free rate of interest; and
σ = standard deviation of the value of assets.

The earnings response coefficient is the change in the value of common equity for a change in earnings represented by e:

$$ERC = \frac{\partial E}{\partial e}. \tag{III}$$

When there are no ESOs:

$$ERC = \frac{\partial E}{\partial e} = \frac{\partial V}{\partial e}. \tag{IV}$$

When there are ESOs (recall that $E = V - nC(V, X, T, r, \sigma)$):

$$ERC = \frac{\partial E}{\partial e} = \frac{\partial V}{\partial e} - \frac{\partial V}{\partial e} n \frac{\partial C}{\partial V} = \frac{\partial V}{\partial e}\left(1 - n\frac{\partial C}{\partial V}\right). \tag{V}$$

Holding all else constant, the ERC is decreasing in the number of shares under option (n) since $0 < (\partial C/\partial V) \leq 1$. This is H1a. Equation (V) also shows that, for a given level of outstanding ESOs, the ERC is a function of the "delta" $(\partial C/\partial V)$ of the options. Hypothesis 1b comes from examining how $(1 - n\ (\partial C/\partial V))$ changes as the value of equity changes:

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[24] The analysis carries through for levered firms as well. The assumptions underlying the analysis are those of the Black and Scholes (1973) model, as well as that (1) the warrants have finite life, and (2) the proceeds from exercise are invested in zero net present value projects.

$$\frac{\partial\left(1 - n\frac{\partial C}{dV}\right)}{\partial E} = -n\frac{\partial^2 C}{\partial V^2} < 0. \qquad \text{(VI)}$$

The dilutive effect of ESOs is greater for firms that have had price increases since $(\partial^2 C/\partial V^2) > 0$.[25] This is H1b.

An additional prediction is that the effect of dilutive securities is asymmetric for good and bad news. Holding the impact of earnings on firm value $(\partial V/\partial e)$ constant, $\partial^2 C/\partial V^2 > 0$ leads to larger ERCs for bad news and smaller ERCs for good news. This predicts an asymmetric response to positive and negative news.

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[25] See, for example, Stoll and Whaley (1993, 225).

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Delayed Security Price Adjustments to Financial Analysts' Forecasts of Annual Earnings

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ABSTRACT: This paper documents that the weighting of analysts' annual earnings forecasts implicit in security prices is lower than the historical relation between financial analysts' forecasts and realized earnings. Short positions in securities in the bottom decile and long positions in the top decile of the cross-sectional distribution of analysts' early-in-the-year earnings forecasts generate significant hedge-portfolio returns in the year after portfolio formation. This delayed price response is more pronounced for firms with relatively low analyst coverage, consistent with the premise that low financial analyst coverage is associated with a variety of factors that impede the information efficiency of the security market. The hedge-portfolio returns concentrate in the months of subsequent quarterly earnings announcements, suggesting that the delayed security price adjustments reflect the market's failure to incorporate information in analysts' forecasts about future earnings, rather than deficiencies in our conditional expectations of security returns.

Keywords: *earnings forecasts; market mispricing; financial analysts' coverage.*

Data Availability: *All data are available from public sources specified in the paper.*

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I. INTRODUCTION

This paper provides evidence that security prices respond with a delay to value-relevant information in analysts' early-in-the-year forecasts of annual earnings. Prior research indicates that financial analysts' annual earnings forecasts are more accurate predictors of realized earnings than are time-series or security-price-based forecasts (Fried and Givoly 1982; Elgers and Murray 1992). Analysts' forecasts, however, do not consistently generate measures of earnings surprise that are more highly associated with contemporaneous security returns. That is, analysts' forecasts are not necessarily better representations of the security market's earnings expectations than are time-series or security-price-based forecasts (O'Brien 1988; Stickel 1991). One may reconcile these apparently contradictory findings in at least two ways: (1) the relative accuracy of financial analysts' forecasts may stem from their ability to predict earnings components that lack contemporary value relevance; and/or (2) the security market's earnings expectations may fail to impound some portion of the value-relevant information in analysts' earnings forecasts.

The evidence in this paper supports the second interpretation. We show that the weighting of analysts' early-in-the-year annual earnings forecasts implicit in security prices is significantly lower than the weighting from historical relations between financial analysts' forecasts and realized earnings. Furthermore, hedge portfolios, based on the cross-sectional distribution of price-scaled analysts' earnings forecasts that are publicly available early in the year, generate significant trading profits (before transaction costs) over the earnings year. This delayed price response is more pronounced for firms with relatively low analyst coverage, consistent with the premise that analyst coverage reflects factors that affect the information efficiency of the security market, such as the level of financial intermediation (Barth and Hutton 2000) or investor sophistication (Walther 1997), or a variety of other institutional features that impede complete arbitrage in security pricing (Bhushan 1994; Hong et al. 2000). Finally, a disproportionately large part of the delayed security price response is concentrated in the months of the subsequent quarterly earnings announcements, suggesting that the delayed security price adjustments reflect the market's failure to incorporate information in analysts' forecasts about future earnings, rather than omitted risk factors or other deficiencies in our conditional expectations of security returns.

Our study contributes to research that evaluates how expectations about future earnings are impounded in security prices. The results show that, for lightly followed firms, security prices fail to reflect efficiently all publicly available information regarding expected value-relevant earnings. At a pragmatic level, our findings identify information—cross-sectional rankings of price-scaled, early-in-the-year financial analysts' annual earnings forecasts—that may be exploited to earn abnormal trading profits (before transaction costs).

From a continuing research perspective, our results add another "anomaly" to an expanding panoply of firm-specific characteristics that require control in assessing relations between accounting disclosures and security prices. In the more specific stream of research on the security market's reaction to earnings surprises, our results indicate that the extent to which security prices underweight analysts' early-in-the-year earnings forecasts varies inversely with the number of analysts covering a firm, and varies directly with the extremity of the forecast in the cross-sectional distribution of price-scaled extant forecasts. Accordingly, the accuracy of analysts' earnings forecasts as a proxy for investors' earnings expectations is likely to depend on both the extremity of the analysts' earnings forecasts and the level of analyst coverage.

The remainder of the paper is organized as follows. Section II summarizes related literature to motivate our hypotheses. Section III describes our research design and reports the empirical results, followed by a summary and concluding remarks in Section IV.

II. PRIOR RESEARCH AND HYPOTHESIS DEVELOPMENT

In this section, we review the recent literature supporting our study's two key hypotheses: (1) the security market inefficiently impounds the information in financial analysts' earnings forecasts made early in the year, and (2) the degree of inefficiency is inversely related to the level of analyst coverage, a proxy for factors that impede the information efficiency of the security market.

Investors' Under- and Overreliance on Analysts' Earnings Forecasts

Several prior studies, which focused mainly on the properties of quarterly earnings forecasts and forecast revisions, suggest that investors underrely on financial analysts' earnings forecasts. Mendenhall (1991) reports a significant positive association between analysts' forecast revisions and abnormal returns around subsequent earnings announcements, which he interprets as evidence that the security market underreacts to the revision signal. Mendenhall (1991) does not, however, examine whether these statistical dependencies between forecast revisions and subsequent returns enable profitable trading strategies. Moreover, positive relations between forecast revisions and subsequent forecast errors (e.g., Elliott et al. 1995) suggest that at least a portion of Mendenhall's (1991) reported dependencies may reflect investor overreliance, rather than underreliance, on analysts' forecasts. That is, analysts' revisions may be directionally correct but insufficient in magnitude. If investors rely on these insufficient revisions in forming their expectations, then overreliance on analysts will lead to predictable errors in market expectations—a positive relation between revisions and subsequent abnormal returns.

Based on an examination of the behavior of security prices, Walther (1997) concludes that investors' earnings expectations do not directly track analysts' quarterly earnings forecasts, and that their degree of reliance on analysts' forecasts relative to time-series predictions appears to increase with analyst coverage, institutional holdings, and firm size. These results, however, do not necessarily imply that the security market misprices the information in analysts' earnings forecasts, because Walther's (1997) tests do not assess the value relevance of the differences in the earnings predictions between time-series and analysts' forecasts. Thus, her evidence does not rule out the possibility that analysts' superior forecast accuracy stems from an ability to predict components of earnings that lack contemporary value relevance.

In contrast to studies suggesting that investors underrely on analysts' earnings forecasts, others provide evidence consistent with investors' overreliance on analysts' forecasts. Bradshaw et al. (2001) report that analysts appear to overestimate the persistence of the accruals component of accounting earnings, which may contribute to a similar bias in investors' earnings expectations documented in Sloan (1996). There is also evidence that investors overreact to analysts' longer-term earnings expectations. La Porta (1996) shows that financial analysts' expected five-year earnings growth rates are too extreme, and that security prices impound these overly extreme projections. Consequently, contrarian strategies that use analysts' growth expectations to form hedge portfolios earn significant abnormal returns.

Overall, these studies suggest disconnects between investors' and financial analysts' earnings expectations, contrary to researchers' widespread use of analysts' forecasts as proxies for investors' expectations. In this study, we investigate whether the security market inefficiently impounds the information in analysts' forecasts of annual earnings. In addition, we explore whether trading rules based on inferred differences between investors' and analysts' earnings expectations are profitable.

Impediments to Complete Arbitrage

If some individual investors fail to weight evidence rationally in pricing securities, then other, more rational investors should quickly exploit, and hence eliminate, any resultant mispricing, *unless* risk or other costs constrain arbitrage strategies. With respect to risk, Black (1986) notes that the price of a stock reflects both the information that informed traders use and the "noise" on which uninformed traders rely. Noise includes information that either is irrelevant to share value or has previously been fully impounded in share prices. Consequently, noise trading may cause share prices to depart temporarily from equilibrium values. The unpredictability of noise traders' expectations increases resale risk in the assets that they trade. Rational arbitrageurs who have short horizons would be concerned about the resale prices of the assets they hold, not just about the present value of dividends. Because of this risk, arbitrageurs may take only limited positions, and mispricing may persist over a substantial period of time.

In addition to risk, trading costs may also constrain arbitrage. Bhushan (1994) provides empirical evidence that institutions seldom trade smaller firms' shares. He argues that per-unit trading costs are higher in smaller firms' shares. Furthermore, large traders need enough liquidity or market depth to exploit their private information, and small firms often lack enough liquidity to support large trades. To the extent that risks and trading costs impede complete arbitrage, we expect that the degree of market inefficiency with respect to analysts' earnings forecasts may vary across firms that are grouped using a proxy for these constraints. The next section explains why we use analyst coverage as a proxy for these constraints in order to partition the sample firms.

Relation between Analyst Coverage and Information Efficiency

Recent evidence indicates that financial analyst coverage is positively associated with the efficiency of investors' information usage. Walther (1997) reports that investors in the securities of lightly followed firms overrely on time-series predictions of earnings, relative to financial analysts' earnings forecasts. Similarly, Bhattacharya (2001) reports that, for firms with little to moderate analyst following, trading around earnings announcements is more closely related to (less accurate) seasonal random-walk forecast errors than to (more accurate) analysts' forecast errors.

Barth and Hutton (2000) posit that analyst coverage can proxy for financial intermediaries' interest in a given firm's securities. They argue that financial intermediation facilitates the pricing efficiency of U.S. capital markets, so that the flows of capital and information are likely to be more efficient for firms with higher analyst coverage. Consistent with this premise, they find that share prices of firms with low or moderate levels of analyst coverage are slower to reflect the value implications of operating cash flows and accruals. Hong et al. (2000) find that momentum-based investment strategies are substantially more profitable for firms with low analyst coverage, because the initial underreaction to value-relevant information, and the subsequent returns momentum, are stronger for these firms.

Broadly, the above studies suggest that financial analyst coverage is associated with institutional and environmental factors that affect the information efficiency of the security market, such as the levels of financial intermediation (Barth and Hutton 2000) or investor sophistication (Walther 1997; Bhattacharya 2001), or other institutional features that impede complete arbitrage in security pricing (Bhushan 1994; Hong et al. 2000). Taken as a whole, existing literature suggests that investors' misweighting of financial analysts' earnings forecasts may lead to predictable abnormal security returns, and that the magnitudes of the misweighting (and security-pricing inefficiencies) are likely to be greater for firms with relatively low analyst coverage.

III. RESEARCH DESIGN AND EMPIRICAL RESULTS

In this section, we first provide descriptive statistics for the sample, which includes all firm-years satisfying our data requirements. We then present the statistical tests in three stages. In the first stage we measure: (1) the historical relation between analysts' earnings forecasts and realized earnings, and (2) the relation between analysts' earnings forecasts and the earnings expectations implicit in security prices. A comparison of these historical and market-inferred weightings indicates whether any systematic differences are consistent with investor over- or underreliance on analysts' forecasts. The results show that, for firm-years with lower than median levels of analyst coverage, the security market's weights on financial analysts' earnings forecasts are significantly less than the historical weights. In the second stage, we show that, for firm-years with lower than median analyst coverage, hedge portfolios based on the levels of analysts' early-in-the-year earnings forecasts subsequently earn significant size-adjusted returns. In the final stage of our testing, we relate the observed abnormal returns to the timing of subsequent earnings news, finding that a disproportionate amount of the abnormal returns occurs in the months of subsequent quarterly earnings announcements.

Sample Selection, Measurement of Variables, and Descriptive Statistics

The empirical analysis uses all December fiscal-year firms available from the intersection of the 1999 Compustat CD-ROM, containing both the Compustat Active and Research firms,[1] and the I/B/E/S Summary Consensus database updated through March 1999, with sufficient information to measure the following primary and control variables (we omit firm-specific subscripts).

Primary Variables

E_{t+1} = actual earnings per share in year t + 1, as reported by I/B/E/S;[2]
FAF_{t+1} = I/B/E/S median consensus forecast of year t + 1 earnings per share, reported in March of year t + 1;
SAR_{t+1} = size-adjusted security return, measured as the realized security return from Compustat, cumulated over 12 months beginning April 1 of year t + 1, less the corresponding mean return for all Compustat firms in the same market-capitalization decile at the start of year t + 1;
FE_{t+1} = analyst forecast error, $E_{t+1} - FAF_{t+1}$; and
ΔE_{t+1} = change in earnings, $E_{t+1} - E_t$.

We scale all per-share variables by share price at the beginning of year t + 1. We use I/B/E/S consensus forecasts rather than individual analyst forecasts because the former are available for a greater number of firm-years in our sample. We use March forecasts because the start of the cumulation period for size-adjusted security returns, and hence the date on which we measure the implicit security-market earnings expectations, is April 1 of the earnings year.[3]

We perform the empirical analyses separately within partitions of relatively lower and higher analyst coverage. We define the two groups by sorting the sample firms in each reporting year based on the number of analysts' forecasts included in the March I/B/E/S consensus. We assign firm-years below the yearly cross-sectional medians to the "lower analyst coverage" group, and the remaining firm-years to the "higher analyst coverage" group. Because analyst coverage is a discrete variable, the groups are not exactly equal in number: lower (higher) analyst coverage groups include 7,724 (8,523) firm-years, respectively.[4]

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[1] It is important to include the Research file to reduce survivorship bias. Specifically, 4,907 of the 16,247 firm-years in our sample are from the Research file.

[2] Using Compustat-reported actual earnings per share before extraordinary items does not affect our inferences.

[3] Our results are robust to the use of I/B/E/S consensus forecasts reported in April or May of the earnings year.

Descriptive statistics for these two groups appear in Table 1. Because our later tests of security-market mispricing do not impose *ex post* adjustments for extreme observations

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[4] We repeated our main tests using three approximately equal partitions to define the groups, and also using two groups after deleting the middle 10 percent of the firm-years. Our inferences are not sensitive to these alternative partitions.

TABLE 1
Descriptive Statistics for Analysis Variables: Medians, Interquartile Ranges, and Rank Correlations (16,247 firm-years, 1982–1998)

Panel A: Firm-Years with Lower Analyst Coverage (n = 7,724)

			Spearman Correlations				
Variable	*Median*	*Interquartile Range*	SAR_{t+1}	FAF_{t+1}	E_{t+1}	FE_{t+1}	ΔE_{t+1}
SAR_{t+1}	−0.042	0.452	1.00				
FAF_{t+1}	0.078	0.051	0.14	1.00			
E_{t+1}	0.066	0.074	0.45	0.53	1.00		
FE_{t+1}	−0.007	0.055	0.43	−0.03	0.74	1.00	
ΔE_{t+1}	0.006	0.057	0.42	0.04	0.55	0.64	1.00
Panel B: Firm-Years with Higher Analyst Coverage (n = 8,523)							
SAR_{t+1}	−0.018	0.347*	1.00				
FAF_{t+1}	0.076	0.044*	0.04*	1.00			
E_{t+1}	0.070*	0.060*	0.30*	0.51	1.00		
FE_{t+1}	−0.002*	0.040*	0.32*	−0.09*	0.71	1.00	
ΔE_{t+1}	0.006	0.031*	0.29*	−0.02	0.51	0.65	1.00

* Differs statistically across low and high analyst coverage groups at a probability level below 0.01, based on intertemporal t-tests of the 17 annual between-group differences.

All descriptive statistics are the averages of the 17 annual measures. We treat each annual measure as a single observation, and base statistical tests on the means and standard deviations of the 17 annual observations to mitigate the effects of cross-sectional dependencies among the firm-year observations (Bernard 1987).

SAR_{t+1} = size-adjusted security return, 12 months beginning April 1 of year t + 1;
FAF_{t+} = I/B/E/S median consensus forecast of earnings per share for year t + 1, reported in March of the earnings year, scaled by share price at the start of the calendar year t + 1;
E_{t+1} = earnings per share in year t + 1 as reported by I/B/E/S, scaled by share price at the start of the calendar year t + 1;
$FE_{t+1} = E_{t+1} - FAF_{t+1}$; and
$\Delta E_{t+1} = E_{t+1} - E_t$.

(e.g., winsorization or deletion of extreme observations), the descriptive statistics in Table 1 report medians, interquartile ranges, and rank-order correlations, rather than means, standard deviations, and Pearson correlations. We report descriptive statistics separately for firm-years with lower (higher) analyst coverage in Panel A (Panel B) of Table 1. Although Table 1 is primarily descriptive, the table indicates statistically significant differences between the analyst coverage groups.

Table 1 shows that the median size-adjusted returns, SAR_{t+1}, are negative in both analyst coverage groups. This occurs because the cross-sectional distribution of security returns is right-skewed, and the size-adjusted returns are measured as the difference between the realized return and the *mean* return for all stocks in the same market capitalization decile. The two analyst coverage groups differ on a variety of dimensions. The lower coverage group exhibits greater cross-sectional dispersion for all of the variables; in addition, analysts are more optimistic for lightly followed firms (more negative forecast error), and these firms have a lower earnings yield. Analysts' earnings forecasts are more highly correlated with subsequent-period SARs in the lower analyst coverage group, consistent with the premise that any security-pricing inefficiencies are likely to be more pronounced for lightly followed firms. Finally, the correlations between (1) size-adjusted returns, and (2) realized earnings, analysts' earnings forecast errors, and earnings changes, are each significantly higher in the lower analyst coverage group. This is consistent with prior studies indicating that security returns anticipate earnings news earlier for larger firms (e.g., Freeman 1987; Collins and Kothari 1989).

Control Variables

To rule out competing explanations for anomalous security returns, we control for variables that proxy for the factors in the Fama and French (1996) (hereafter Fama-French) three-factor asset-pricing model, as well as other variables that prior studies find to be associated with subsequent abnormal returns. We obtain firm-specific estimates of the factor sensitivities (or loadings) in the Fama-French three-factor model by estimating the following time-series regression for each firm:

$$(R_t - R_{ft}) = \alpha + b(R_{mt} - R_{ft}) + s(SMB_t) + h(HML_t) + \varepsilon_t, \qquad (1)$$

where R_t is the realized return for a specific firm, R_{ft} is the corresponding risk-free rate, and R_{mt} is the market return, in period t; b, s, and h represent (respectively) the firm's sensitivity to the excess returns on a broad market portfolio ($R_{mt} - R_{ft}$), the difference in returns to portfolios of small and large stocks (SMB), and the difference in returns to portfolios of high and low book-to-market stocks (HML).[5] Fama and French (1996) report that a wide variety of previously documented security-returns anomalies largely disappear when returns are measured using this three-factor model. We also include the intercept estimate, α, as a separate control variable, based on Jain's (1986) argument that the firm-specific average effect of any additional (missing) factors will be impounded in the intercept.

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[5] We compute monthly measures of the three independent variables as in Fama and French (1996, legend to Table 1). We obtain the firm-specific sensitivity coefficients by estimating the time-series regression in equation (1) for each sample firm in the 60 months preceding the analysis year. We use the CRSP value-weighted index to measure R_{mt}, and R_f is the one-month Treasury bill rate observed at the beginning of the month. The SMB returns are based on the cross-sectional median market equity for NYSE stocks, in each year. The HML returns are based on the cross-sectional rankings of the values of book-to-market equity for NYSE stocks, in each year. We calculate value-weighted monthly returns on the portfolios for every month in the sample.

In addition to these three factor sensitivities, we control for the following five firm-specific characteristics that earlier research found to be associated with subsequent abnormal security returns: (1) prior-year earnings scaled by share price (Basu 1977); (2) prior-year accruals scaled by average assets (Sloan 1996); (3) size-adjusted security returns for the 12 months preceding the analysis year, based on Jegadeesh and Titman's (1993) finding that short-run returns tend to continue in the subsequent year;[6] (4) five-year expected growth rate in earnings (La Porta 1996); and (5) book value to market value of common equity (Fama and French 1992). A total of 16,247 firm-years had data for the analysis and control variables described above.

Table 2 contrasts the medians and interquartile ranges of the four multifactor and five anomaly control variables, across the low and high analyst coverage groups. The ranges of seven of the nine attributes are significantly wider for the more lightly followed firms. In addition, the median values of all the variables except prior-year size-adjusted returns and the intercept estimate from equation (1) above differ significantly across the analyst coverage groups. Lightly covered firms are significantly smaller, as expected. Overall, the descriptive statistics in Tables 1 and 2 indicate that grouping firm-years by financial analyst coverage partitions the sample on a variety of dimensions associated with security-pricing inefficiencies and with other factors that are systematically priced in the security market.

Inferring Security-Market Weightings of Financial Analysts' Annual Earnings Forecasts

To compare the weighting of financial analysts' earnings forecasts implicit in security prices to the historical relation between analysts' forecasts and realized earnings, we first empirically infer the weightings implicit in security prices. Recent studies have used Mishkin's (1983) framework to examine the market's weighting of accounting earnings variables (Sloan 1996; Barth and Hutton 2000; Bradshaw et al. 2001; Xie 2001). Following the general approach used in these studies, we assess the historical linear relation between earnings, E_{t+1}, and a given predictor variable, X_1, using the following cross-sectional regression (firm subscripts omitted):

$$E_{t+1} = \beta_0 + \beta_1 X_1 + \varepsilon_{t+1}. \tag{2}$$

To measure the implicit linear weighting of this same predictor variable in the market's earnings expectations, we begin with a typical specification of the relation between abnormal returns and unexpected earnings:

$$SAR_{t+1} = \alpha_0 + \alpha_1 UE_{t+1} + \varepsilon_{t+1}, \tag{3}$$

where SAR_{t+1} represents abnormal (size-adjusted) security returns, and UE_{t+1} represents unexpected earnings in year t + 1. The unexpected earnings variable, UE_{t+1}, is decomposed into realized earnings, E_{t+1}, and expected earnings, $E(E_{t+1})$:

$$UE_{t+1} = E_{t+1} - E(E_{t+1}). \tag{4}$$

We substitute the linear prediction of E_{t+1} in equation (2) for expected earnings $E(E_{t+1})$ embedded in equation (3), and rearrange equation (3) as follows:

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[6] The short-term continuation of abnormal returns was the only one of a variety of asset-pricing anomalies that Fama and French (1996) were unable to displace by using a three-factor returns model.

TABLE 2
Descriptive Statistics for Fama-French Multifactor Model Parameters and Anomaly Control Variables: Medians, Interquartile Ranges, and Between-Group Comparisons (16,247 firm-years, 1982–1998)

	Lower Analyst Coverage (n = 7,724)		*Higher Analyst Coverage (n = 8,523)*	
	Median	*Interquartile Range*	*Median*	*Interquartile Range*
Fama-French Parameters[a]				
Intercept (α)	0.004	0.031	0.006	0.020*
Market sensitivity (b)	0.943	0.931	1.005*	0.628
Size sensitivity (s)	0.717	1.149	0.143*	0.902*
Book-to-market sensitivity (h)	0.077	1.182	0.137*	0.930*
Anomaly Control Variables				
EP_t	0.064	0.052	0.067*	0.051
$Accrual_t$	−0.030	0.082	−0.043*	0.056*
SAR_t	−0.076	0.474	−0.008	0.357*
$Growth_{t+1}$	15.3%	10.1%	12.0%*	7.3%*
B/M_t	0.613	0.463	0.562*	0.419*
Other Measures				
MVE_t	$154.6	$272.7	$1,385.7*	$2,736.1*
Analyst coverage	3.9	3.3	15.6*	9.9*

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* Differs statistically across low and high analyst coverage groups at a probability level below 0.01, based on intertemporal t-tests of the 17 annual between-group differences.

[a] Intercept and factor sensitivities are obtained by estimating the Fama and French (1996) three-factor asset-pricing model. For each firm, we estimate the following time-series regression for the sixty months preceding the start of the analysis year:

$$(R_t - R_{ft}) = \alpha + b(R_{mt} - R_{ft}) + s(SMB_t) + h(HML_t) + \varepsilon_t,$$

where R_t is the realized return for a specific firm, R_{ft} is the corresponding risk-free rate, and R_{mt} is the market return in period t; b, s, and h represent the firm's sensitivity to the excess returns on a broad market portfolio ($R_{mt} - R_{ft}$), the difference in returns to portfolios of small and large stocks (SMB), and the difference in returns to portfolios of high and low book-to-market stocks (HML), respectively.

EP_t = earnings per share in year t as reported by I/B/E/S, scaled by share price at the start of year t;
$Accrual_t$ = aggregate accruals scaled by average assets in year t;
SAR_t = size-adjusted security return, 12 months beginning April 1 of year t;
$Growth_{t+1}$ = I/B/E/S consensus three- to five-year expected rate of growth in earnings in March of year t + 1;
B/M_t = ratio of book-to-market value of common equity, end of year t;
MVE_t = total market value of outstanding common shares (millions of dollars), last trading day of year t; and
Analyst coverage = number of analysts' annual earnings forecasts of year t + 1 earnings included in the March t + 1 I/B/E/S report.

$$SAR_{t+1} = \alpha_0 + \alpha_1[E_{t+1} - E(E_{t+1})] + \varepsilon_{t+1} \quad (5)$$

$$= \alpha_0 + \alpha_1 E_{t+1} - \alpha_1(\beta_0^* + \beta_1^* X_1) + \varepsilon_{t+1} \quad (5a)$$

$$= \phi_0 + \alpha_1 E_{t+1} + \phi_1 X_1 + \varepsilon_{t+1}, \quad (5b)$$

where β_1^* represents the slope parameter of the linear relation of X_1 to the security market's expectations, $E(E_{t+1})$.[7] Empirical estimation of equation (5b) yields estimates of α_1 and ϕ_1. Then, $\hat{\beta}_1^* = -\hat{\phi}_1/\hat{\alpha}_1$.[8]

Market efficiency requires that the market's weighting of the predictor variable in equation (5a) be consistent with the historical relation from equation (2). Formally, this can be stated as:

$$\beta_1^* = \beta_1.$$

Comparing the historical weights from equation (2) with the market's weights implicit in equation (5a) requires testing the hypothesis that β_1 (from equation 2) $= -\phi_1/\alpha_1$ (from equation 5b).[9]

In Mishkin's (1983) framework, it is not necessary that equation (2) be a complete specification of the relation between predictor variables and subsequent earnings. Rather, it is necessary only to use the same functional form and set of predictor variables in estimating both equations (2) and (5b) to compare parameters across the two equations. However, this framework does assume that security prices are efficient with respect to all variables that are omitted from the equations and are correlated with X_1. If security prices, SAR_{t+1}, include a delayed response to information in correlated omitted variables, then the coefficient ϕ_1, estimated in equation (5b), will impound both the security market's delayed adjustment to the correlated omitted variables and the market's weighting of X_1. Accordingly, the estimating equation must control for potentially correlated variables:

$$SAR_{t+1} = \lambda_0 + \lambda_1 E_{t+1} + \lambda_2 X_1 + \sum_{i=2}^{n} \theta_i X_i + \varepsilon_{t+1}, \quad (6)$$

where X_i, $i = 2, n$ are the additional variables having delayed security-price effects. Moreover, if the additional variables included in equation (6) are potentially correlated with E_{t+1} and X_1, then one must also add them to equation (2) in estimating the historical relation between analysts' forecasts and realized earnings. In this case, cross-equation comparisons of parameter estimates require that both equations include the same set of X_i's.

Relations between Financial Analysts' Earnings Forecasts and (1) Realized Earnings and (2) Security Market Earnings Expectations

Our initial tests compare the historical relation between financial analysts' earnings forecasts and realized earnings, to the relation between financial analysts' earnings forecasts and the earnings expectations implicit in security prices. To estimate these relations, we specify X_1 in equations (2) and (6) as financial analysts' annual earnings forecasts, FAF_{t+1}.

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[7] The Appendix relates this specification to the use of earnings levels and changes in estimating the return/earnings relation.

[8] The intercept, $\hat{\phi}_0 = \hat{\alpha}_0 - \hat{\alpha}_1\hat{\beta}_0^*$. This intercept value has no bearing on the issues addressed in the paper.

[9] Previous studies (e.g., Sloan 1996; Xie 2001) use iterative weighted nonlinear least squares to test cross-equation constraints on the coefficient estimates in equations (2) and (5b). We base our empirical inferences instead on intertemporal tests of vectors of parameters obtained in yearly estimates, to avoid distributional issues encountered in cross-sectional and pooled estimation (e.g., cross-correlations in regression residuals).

We also winsorize the extreme 1 percent of the values of SAR_{t+1}, FAF_{t+1}, and E_{t+1} in each tail of the cross-sectional distributions of these variables. To mitigate the potential impact of extreme values, we replace each control and anomaly variable with its scaled decile value.[10] Panel A of Table 3 reports the historical relation between realized earnings, E_{t+1}, and the I/B/E/S March consensus forecast, FAF_{t+1}, obtained by estimating the following cross-sectional regression, an analogue of equation (2), in each year, 1982–1998:

$$E_{t+1} = \beta_0 + \beta_1 FAF_{t+1} + \sum_{i=1}^{4} \phi_i Factor_i^{dec} + \sum_{j=1}^{5} \gamma_j Anomaly_j^{dec} + \varepsilon_{t+1}. \tag{7}$$

Equation (7) includes the scaled decile values of the estimates of the four Fama-French factors (the $Factor_i$) and the five anomaly control variables (the $Anomaly_j$), denoted by the "dec" superscript, to enable us to compare coefficients across equations. The purpose in Table 3 is to assess the historical and the market-inferred weightings of the analysts' forecasts, so we do not use the scaled decile transformations for the earnings and forecast variables because this would sacrifice the economic interpretation of the parameter estimates. However, our later tests employ scaled decile transformations of *all* independent variables.

Equation (7) focuses on the relation between analysts' forecasts and realized earnings, and the results appear in Panel A of Table 3. As expected, the estimated relation is reliably positive. The intertemporal t-statistics indicate significance at probability levels below 0.01 in both analyst coverage groups, and the estimated slope coefficients are positive in all 17 years.

Panel B of Table 3 assesses the weightings of analysts' forecasts implicit in the security market's earnings expectations, by estimating the following relation, an analogue of equations (5b) and (6):

$$SAR_{t+1} = \alpha_0 + \alpha_1 E_{t+1} + \alpha_2 FAF_{t+1} + \sum_{i=1}^{4} \delta_i Factor_i^{dec} + \sum_{j=1}^{5} \varphi_j Anomaly_j^{dec} + \nu_{t+1}. \tag{8}$$

The estimation results show that the implicit security-market weightings of the analysts' earnings forecasts, $-\alpha_2/\alpha_1$, are positive in both the lower (0.070) and higher (0.690) analyst coverage groups, as expected. However, the weightings are significantly positive only in the higher analyst coverage group.

A formal contrast of these historical and implicit security-market weightings of the analysts' forecasts appears in Panel C of Table 3. The historical (β_1) and market ($-\alpha_2/\alpha_1$) weightings differ significantly *only* in the lower analyst coverage group. For the lightly followed firms, the historical weighting (0.962) is significantly higher than the implicit market weighting (0.070) at a probability level lower than 0.01. This is consistent with our expectation that for lightly followed firms, the security market misweights information in analysts' early-in-the-year forecasts of annual earnings.

Value Relevance of Differences in Financial Analysts' and Security-Market Earnings Expectations

The previous section documents that, for the lightly followed firms, the historical relation between financial analysts' earnings forecasts and realized earnings is significantly

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[10] We sort on the variables in each year and assign the firm-year observations to deciles. As in Abarbanell and Bushee (1998), we replace the value of each variable by its scaled decile rank, varying from 0 to 9. The ranks are scaled by 9, so the scaled decile values range from 0 to 1.

TABLE 3
Relations between Financial Analysts' Forecasts and: (1) Realized Earnings, and (2) Earnings Expectations Implicit in Security Prices, after Controlling for Multifactor Asset Pricing and Other Anomalies[a]
(16,247 firm-years, 1982–1998)

Panel A: Relation between Realized Earnings and Financial Analysts' Forecasts and Control Variables

$$\text{Model: } E_{t+1} = \beta_0 + \beta_1 FAF_{t+1} + \sum_{i=1}^{4} \phi_i Factor_i^{dec} + \sum_{j=1}^{5} \gamma_j Anomaly_j^{dec} + \varepsilon_{t+1}$$

	Lower Analyst Coverage (n = 7,724)			*Higher Analyst Coverage (n = 8,523)*		
	β_0^L	β_1^L	R^2_a	β_0^H	β_1^H	R^2_a
Coefficient	−0.055	0.962	0.39	−0.044	0.804	0.35
(t-statistic)	(−5.19)*	(21.88)*		(−5.83)*	(14.32)*	
Years positive	2/17	17/17*		0/17*	17/17*	

Panel B: Relation between Earnings Expectations Implicit in Security Prices and Financial Analysts' Forecasts and Control Variables

$$\text{Model: } SAR_{t+1} = \alpha_0 + \alpha_1 E_{t+1} + \alpha_2 FAF_{t+1} + \sum_{i=1}^{4} \delta_i Factor_i^{dec} + \sum_{j=1}^{5} \varphi_j Anomaly_j^{dec} + \nu_{t+1}$$

	Lower Analyst Coverage (n = 7,724)				*Higher Analyst Coverage (n = 8,523)*			
	α_0^L	α_1^L	α_2^L	R^2_a	α_0^H	α_1^H	α_2^H	R^2_a
Coefficient	−0.010	1.364	−0.136	0.17	0.000	0.981	−0.778	0.15
(t-statistic)	(−0.27)	(12.40)*	(−0.80)		(0.00)	(11.41)*	(−3.70)*	
Years positive	9/17	17/17*	8/17		11/17	17/17*	1/17*	
Implicit weight:		$-\alpha_2^L/\alpha_1^L = 0.070$				$-\alpha_2^H/\alpha_1^H = 0.690$		

Panel C: Contrasts of Historical and Security Market-Inferred Weightings of Financial Analysts' Forecasts

Lower Analyst Coverage	*Higher Analyst Coverage*
$H_0: \beta_1^L = -\alpha_2^L/\alpha_1^L$	$H_0: \beta_1^H = -\alpha_2^H/\alpha_1^H$
Difference = 0.892 (t = 6.98)*	Difference = 0.114 (t = 0.72)

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* Significant at a probability below 0.01, based on intertemporal tests (see note to Table 1).

[a] We obtained the parameter estimates reported in this table after winsorizing the 1 percent of observations in the cross-sectional tails of the distributions of E_{t+1}, FAF_{t+1}, and SAR_{t+1}.

E_{t+1} = earnings per share in year t + 1 as reported by I/B/E/S, scaled by share price at the start of the calendar year t + 1;

FAF_{t+1} = I/B/E/S median forecast of earnings per share for year t + 1, reported in March of the earnings year, scaled by share price at the start of the calendar year t + 1;

SAR_{t+1} = size-adjusted security return, 12 months beginning April 1 of year t + 1;

$Factor_i^{dec}$, $_{i=1,4}$ = scaled decile values for the intercept and three slope parameters of the Fama-French multifactor asset pricing model, described in Table 2 (coefficient estimates not tabulated); and

$Anomaly_j^{dec}$, $_{j=1,5}$ = scaled decile values for the five anomaly control variables described in Table 2 (coefficient estimates not tabulated).

higher than the inferred weighting of analysts' earnings forecasts implicit in security prices. This weighting difference, however, does not necessarily imply that the market inefficiently impounds the information in analysts' forecasts. For example, the historical relations may reflect analysts' ability to predict some components of earnings that lack contemporary valuation relevance. If so, then the market weightings would not reflect those earnings components. In this case, the market weightings would differ from the historical weightings, but the differences would not be due to mispricing by the security market. On the other hand, delayed adjustment of security prices to the valuation information in the earnings predictors would be consistent with market mispricing.

The analysis reported in this section tests for delayed security-price adjustments to the information in financial analysts' earnings forecasts. In each year, we sort firms based on the magnitude of the share-price-scaled March analysts' earnings forecasts. We assign the firm-years to ten equal-size portfolios. The same procedure is applied to each of the five anomaly control variables described earlier (in Table 2). Panel A of Table 4 reports the mean size-adjusted returns over the 12 months after the forecast month for firms in the extreme (bottom and top) earnings-forecast decile portfolios, as well as for firms in the extreme decile portfolios for each of the five anomaly control variables. Panel A of Table 4 also reports the hedge-portfolio returns accruing to short positions in the bottom deciles and long positions in the top deciles of the forecast and anomaly control variables.

The results in Panel A of Table 4 indicate that, when the hedge portfolio is formed based on the top and bottom deciles of the magnitude of analysts' forecasts, firm-years with lower analyst coverage earn size-adjusted hedge-portfolio returns of 17.6 percent ($t = 4.99$, significant at a probability value below 0.01).[11] The directional difference in the returns to the extreme portfolios is consistent with the difference in historical and security-market weightings of analysts' forecasts observed in Table 3. That is, the negative returns in the bottom portfolio and the positive returns in the top portfolio are consistent with the market's initial underreaction to the most extreme analysts' earnings forecasts.[12] Each of the five anomaly control variables also leads to hedge-portfolio returns that are directionally consistent with prior research, although only the hedge returns to operating accruals and to prior-year returns are significant at a probability below 0.01.

Panel A of Table 4 also shows that the hedge-portfolio returns for firm-years with higher analyst coverage are not statistically significant, except for the accrual anomaly. These results are consistent with prior evidence that pricing anomalies are more apparent for smaller, less heavily followed firms (e.g., Foster et al. 1984; Bartov et al. 2000).

The unconditional hedge-portfolio returns reported in Panel A of Table 4 do not control for overlap among the different variables and decile classifications. To show that the relation between financial analysts' forecasts and subsequent abnormal returns is distinct from the other anomalies, Panel B of Table 4 reports the *incremental* returns to the financial analysts' forecasts and each of the control variables. We estimate the incremental returns using the following model:

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[11] Although Table 4 reports results only for the extreme (bottom and top) portfolios, the variation in returns across the intermediate deciles of the analysts' earnings forecasts is nearly monotonic; i.e., the mean returns generally increase with the decile number.

[12] As a test of robustness to the inclusion of potentially stale forecasts in the March consensus, we re-measure the hedge-portfolio returns based on deciles of consensus earnings forecasts reported in the April and May I/B/E/S reports. The hedge returns are very similar to those reported in Table 4. For the lower analyst coverage firms, the hedge returns are 15.5 percent (18.3 percent) based on April (May) forecasts, with t-statistics of 4.54 (5.37). The hedge returns in the higher analyst coverage group are not significant in either month. In addition, the Spearman correlations between analysts' forecasts (FAF_{t+1}) and realized earnings (E_{t+1}) in Table 1 are very similar for the higher (0.51) and lower (0.53) analyst coverage groups. Thus, there is no evidence of differential staleness across the two groups.

TABLE 4
Size-Adjusted Security Returns to Hedge Portfolios for Extreme Deciles of Financial Analysts' Earnings Forecasts and Anomaly Control Variables
(16,247 firm-years, 1982–1998)

Panel A: Unconditional Relations between Variables' Deciles and Year t + 1 Size-Adjusted Returns (SAR_{t+1})

	FAF_{t+1}	EP_t	$Accrual_t$	SAR_t	$Growth_{t+1}$	B/M_t
Lower analyst coverage (n = 7,724)						
Bottom decile	−0.140	−0.053	0.013	−0.082	0.001	−0.016
Top decile	0.036	0.027	−0.094	0.041	−0.038	0.007
Hedge	+0.176	+0.080	−0.107	+0.123	−0.039	+0.023
(t-statistic)	(4.99)*	(2.27)	(−3.50)*	(4.29)*	(−1.21)	(0.57)
Years positive	15/17*	12/17	2/17*	15/17*	8/17	9/17
Higher analyst coverage (n = 8,523)						
Bottom decile	−0.016	0.054	0.016	−0.054	−0.003	−0.024
Top decile	0.019	0.010	−0.050	0.028	−0.051	0.015
Hedge	+0.035	−0.044	−0.066	+0.082	−0.048	+0.039
(t-statistic)	(1.27)	(−1.68)	(−1.92)	(2.15)	(−.84)	(1.16)
Years positive	11/17	8/17	2/17*	9/17	6/17	11/17

Panel B: Incremental Relations: Multiple Regressions of Year t + 1 Size-Adjusted Returns on Scaled Variables' Deciles

$$\text{Model: } SAR_{t+1} = \phi_0 + \phi_1 FAF^{dec}_{t+1} + \phi_2 EP^{dec}_t + \phi_3 Accrual^{dec}_t + \phi_4 SAR^{dec}_t + \phi_5 Growth^{dec}_{t+1} + \phi_6 B/M^{dec}_t + \sum_{i=1}^{4} \delta_i Factor^{dec}_t + \varepsilon_{t+1}$$

		FAF^{dec}_{t+1}	EP^{dec}_t	$Accrual^{dec}_t$	SAR^{dec}_t	$Growth^{dec}_{t+1}$	B/M^{dec}_t
	ϕ_0	ϕ_1	ϕ_2	ϕ_3	ϕ_4	ϕ_5	ϕ_6
Lower analyst coverage (n = 7,724)							
$\hat{\phi}_i$	−0.080	0.127	−0.013	−0.104	0.093	−0.000	0.012
(t-statistic)	(−2.15)	(3.40)*	(−0.42)	(−5.83)*	(5.17)*	(−0.00)	(0.44)
Years positive	5/17	15/17*	8/17	1/17*	15/17*	9/17	9/17
Higher analyst coverage (n = 8,523)							
$\hat{\phi}_i$	−0.038	−0.001	0.032	−0.060	0.066	0.032	0.011
(t-statistic)	(−1.23)	(−0.04)	(1.15)	(−3.16)*	(2.95)*	(0.96)	(0.46)
Years positive	6/17	10/17	8/17	3/17*	15/17*	8/17	9/17

(*Continued on next page*)

TABLE 4 (Continued)

Panel C: Residual Size-Adjusted Returns to Hedge Portfolios

$$\textit{Model: } SAR_{t+1} = \gamma_0 + \sum_{i=1}^{4} \gamma_i Factor_i^{dec} + \sum_{j=1}^{5} \phi_j Abnomaly_j^{dec} + residual_{t+1}$$

	Residual Returns	
	Lower Analyst Coverage ($n = 7{,}724$)	*Higher Analyst Coverage* ($n = 8{,}523$)
Bottom forecast decile	−0.116	−0.013
Top forecast decile	+0.006	0.000
Hedge	+0.122	+0.013
(t-statistic)	(5.68)*	(1.16)
Years positive	16/17*	12/17

* Significant at a probability below 0.01, based on intertemporal tests (see note to Table 1).

SAR_{t+1} = size-adjusted security return, 12 months beginning April 1 of year t + 1;
FAF_{t+1}^{dec} = I/B/E/S consensus earnings forecast in March of year t + 1, scaled by share price, transformed to a scaled-decile variable varying in value from 0 to 1;
EP_t^{dec} = earnings-to-price ratio in year t, transformed to a scaled-decile variable varying in value from 0 to 1;
$Accrual_t^{dec}$ = aggregate accruals scaled by average assets in year t, transformed to a scaled-decile variable varying in value from 0 to 1;
SAR_t^{dec} = size-adjusted security return, 12 months beginning April 1 of year t, transformed to a scaled-decile variable varying in value from 0 to 1;
$Growth_{t+1}^{dec}$ = I/B/E/S consensus three- to five-year expected rate of growth in earnings in March of year t + 1, transformed to a scaled-decile variable varying in value from 0 to 1;
B/M_t^{dec} = book-to-market-value ratio, end of year t, transformed to a scaled-decile variable varying in value from 0 to 1;
$Factor_i^{dec}, {}_{i=1,4}$ = scaled decile values for the intercept and slope parameters of the Fama-French multifactor asset pricing model, described in Table 2; and
$Anomaly_j^{dec}, {}_{j=1,5}$ = scaled decile values for the five anomaly control variables (EP, Accrual, SAR, Growth, and B/M).

$$SAR_{t+1} = \phi_0 + \phi_1 FAF_{t+1}^{dec} + \phi_2 EP_t^{dec} + \phi_3 Accrual_t^{dec} + \phi_4 SAR_t^{dec} + \phi_5 Growth_{t+1}^{dec} + \phi_6 B/M_t^{dec} + \sum_{i=1}^{4} \delta_i Factor_t^{dec} + \varepsilon_{t+1}. \tag{9}$$

We measure all of the independent variables in equation (9) as scaled-decile ranks, using the procedure described in footnote 10. This transformation reduces the influence of extreme observations. The slope coefficient is interpretable as the incremental size-adjusted returns to a hedge consisting of a short position in the bottom decile, and a long position in the top decile, of each independent variable after controlling for the other variables.

The results reported in Panel B of Table 4 indicate that the relation between delayed price adjustments and analysts' earnings forecasts persists after controlling for these potential confounding variables. In the lower analyst coverage group, controlling for the five anomaly and four Fama-French factors reduces the incremental return to the analysts' forecast-based hedge portfolio to 12.7 percent (t = 3.40). Fifteen out of the 17 years have positive hedge-portfolio returns. In the higher analyst coverage group, the forecast-based hedge portfolio returns are insignificant.[13]

As a test of robustness with respect to the earnings-price anomaly, we replace the forecast-level variable, FAF_{t+1}, with a forecast-change variable, $(FAF_{t+1} - E_t)$, and reestimate equation (9). The untabulated results are very similar to those based on the forecast level in Panel B of Table 4. For firm-years with lower analyst coverage, the estimated coefficient on the forecast change variable is 12.5 percent ($t = 4.77$), and the coefficient estimate is positive in 16 out of 17 years. For firm-years with higher analyst coverage, the hedge-portfolio returns are not statistically significant.

Equation (9) controls for multiple attributes that potentially are associated with analysts' forecasts and security returns. The model assumes, however, that the relation between the scaled forecast deciles and size-adjusted returns is linear in the annual cross-sections. We relax this assumption in Panel C of Table 4, by re-measuring the abnormal returns as the residuals from estimating the following model in each year:[14]

$$SAR_{t+1} = \gamma_0 + \sum_{i=1}^{4} \gamma_i Factor_i^{dec} + \sum_{j=1}^{5} \phi_j Anomaly_j^{dec} + residual_{t+1}. \qquad (10)$$

The results, reported in Panel C of Table 4, reinforce our inferences: for lightly followed firms, hedge portfolios that are long in the top analysts' earnings-forecast decile, and short in the bottom decile, earn statistically significant size-adjusted returns. The estimated return is 0.122 ($t = 5.68$), very close to the cross-sectional coefficient of 0.127 ($t = 3.40$) estimated for the analyst forecast variable (Panel B of Table 4). Similar to the earlier results, there is no significant incremental association between hedge-portfolio returns and analysts' forecast deciles for firms with higher analyst coverage.

The lower analyst coverage group hedge-portfolio returns reported in Panels A and C of Table 4 are clearly asymmetric. Panel A shows a negative 14.0 percent return for the bottom forecast decile, and only a 3.6 percent return for the top forecast decile (in Panel C the corresponding abnormal returns are −11.6 and +0.6 percent, respectively). Similar asymmetries exist for each of the anomaly control variables reported in Panel A; i.e., most of the abnormal returns to the hedge portfolios are attributed to the short-sale side of the hedge. This is similar to the asymmetry in returns to accruals-based portfolios reported in Sloan (1996), and is consistent with the argument that the costs and risks of short sales may mitigate correction of security-pricing inefficiencies (Lee et al. 1991).

Relations of Delayed Security Returns to Subsequent Earnings Announcements

Table 4 documents a relation between analysts' earnings forecasts and subsequent size-adjusted returns. If our delayed pricing results are attributable to the market's failure to assimilate information about future earnings from analysts' forecasts until that information is revealed by subsequent earnings announcements, then the abnormal returns we observed in Table 4 should concentrate in periods when those earnings subsequently become known to investors. For this reason, our final tests focus on the relative proportions of those returns that occur in quarterly earnings announcement and non-announcement months.

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[13] Variance inflation factors for the FAF_{t+1} vary intertemporally from 1.11 to 1.24, well below the usual threshold of 10.0 for serious multicollinearity (Neter et al. 1989).

[14] This test is the statistical equivalent of a one-factor analysis of covariance, where the control variables are covariates, and the treatments are the bottom and top forecast deciles. This specification assumes that size-adjusted returns are linearly related to the scaled decile ranks of the control variables.

TABLE 5
Comparison of Size-Adjusted Returns across Extreme Analyst Forecast Deciles in Earnings Announcement and Non-Announcement Months
(10,007 firm-years, 1988–1998)

	Average Monthly Size-Adjusted Returns			
	Lower Analyst Coverage (n = 4,681)		*Higher Analyst Coverage (n = 5,326)*	
	Announcement Months	*Non-Announcement Months*	*Announcement Months*	*Non-Announcement Months*
Bottom forecast decile	−0.0080	−0.0098	0.0003	−0.0039
Top forecast decile	0.0055	−0.0047	−0.0001	−0.0085
Hedge	+0.0135	+0.0051	−0.0004	−0.0046
(t-statistic)	(2.55)*	(1.62)	(−0.11)	(−1.20)
Years positive	9/11*	7/11	4/11	5/11

* Significant at a probability below 0.05, based on intertemporal tests (see note to Table 1).

The Compustat data available to us include announcement dates for quarterly earnings over the period 1988–1998 for 10,007 of the sample firm-years. Quarterly earnings announcement dates are available for 4,681 of the firm-years in the lower analyst coverage group, and for 5,326 of the firm-years in the higher analyst coverage group.[15] Within each of the analyst coverage groups, we compare the forecast-based hedge-portfolio size-adjusted returns realized in earnings announcement calendar months to those realized in non-announcement months.[16] Table 5 shows that, for the firm-years with higher analyst coverage, the hedge-portfolio returns do not differ significantly from zero in announcement or non-announcement months. For lower coverage firms, the hedge-portfolio returns differ significantly from zero only in the earnings announcement months (t = 2.55, probability below 0.05). In earnings announcement months, the average size-adjusted return is 1.35 percent, compared to just 0.51 percent in non-announcement months.[17] This disproportionate concentration of size-adjusted returns in earnings announcement months supports our interpretation that the association between size-adjusted returns and price-scaled analysts' earnings forecasts is due, at least in part, to bias in investors' earnings expectations.

IV. SUMMARY AND CONCLUSIONS

This paper shows empirically that for firms with relatively low financial analyst coverage, the weighting of analysts' annual earnings forecasts that is implicit in security prices is significantly less than the historical relations between financial analysts' forecasts and realized earnings. Our evidence that analysts' beginning-of-year annual earnings forecasts are associated with abnormal security returns subsequently accumulated over the earnings year is consistent with a delayed price reaction to the value-relevant information in the analysts' beginning-of-year forecasts. A simple hedge-portfolio strategy, with short (long) positions in securities in the bottom (top) deciles of the cross-sectional distribution of analysts' earnings forecasts early in the earnings year, generates statistically significant trading profits in the year after portfolio formation for firms with relatively low analyst coverage. Finally, for lightly covered firms, the major portion of the abnormal returns is concentrated in the months of quarterly earnings announcements after the analysts' initial forecasts, consistent with the delayed security-price adjustments' reflecting earnings news, rather than omitted risk factors or other deficiencies in our conditional expectations of security returns.

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[15] We continue to group the firm-years based on the original sample's median analyst following.

[16] As a programming convenience, we use calendar-month size-adjusted returns. The sample-wide average earnings announcement date is approximately the 25th day of the month, so this convenience should have no substantive effect on our inferences.

[17] The average monthly hedge-portfolio returns reported in Table 5 during the four announcement and the eight non-announcement months for the lower coverage firms yield an annual hedge-portfolio return of about 9.89 percent, which is less than the annual hedge-portfolio return of 17.6 percent for the larger sample of 7,724 firm-years reported in Panel A of Table 4. This difference in returns between the two tables occurs because the requirement that quarterly earnings announcement dates be available has skewed the analysis in Table 5 toward the larger firms.

These findings refine our understanding of the manner in which investors impound expectations about future earnings in security prices. For firms with lower analyst coverage, security prices do not reflect efficiently all of the value-relevant information in the publicly available earnings forecasts of a prominent set of financial intermediaries, i.e., the financial analysts included in the March I/B/E/S consensus forecasts. Although our results suggest that a simple trading strategy may be based on the cross-sectional rankings of early-in-the-year price-scaled financial analysts' annual earnings forecasts, our measured abnormal returns are before transaction costs that an investor would incur in implementing the designated investment strategy. Given that a substantial portion of our reported abnormal returns arise from short sales, future research might assess whether these abnormal returns are sufficient to compensate investors for the transaction costs and risks associated with short sales.

Our study suggests that price-scaled analysts' earnings forecasts constitute another "anomaly" variable that subsequent research should consider in assessing relations between accounting disclosures and security prices. In addition, our results indicate that the ability of analysts' early-in-the-year earnings forecasts to represent the earnings expectations embedded in security prices is less for lightly followed firms, and for firms with extreme price-scaled earnings forecasts.

A limitation of this paper is that we do not propose a theory of investor behavior that would explain our findings. Such a theory would need to reconcile our evidence of investor underreliance on analysts' early-in-the-year annual earnings forecasts to prior literature reporting investor overreliance on other dimensions of analysts' forecasts—in particular, the apparent overreliance on analysts' long-term growth forecasts documented by La Porta (1996).

APPENDIX

A Market-Inferred Linear Transformation of the Expected Earnings Proxy

The specifications of the returns/earnings relation employed in the body of the paper (equations [3] through [5b]) remove a coefficient constraint that is implicit in equations based on earnings changes, or other measures of "unexpected earnings." To illustrate, note that estimation of equation (3) with earnings changes as a proxy for UE_{t+1} implicitly constrains β_1^* to equal 1.0.[18] Estimation of equation (5b) is less restrictive and allows the security market's earnings expectation to be proxied by any linear transformation of the cross-section of prior-year earnings (because ϕ_1 is not constrained to equal $-\alpha_1$). Equation (5b) is statistically identical to a "levels-and-changes" specification of the returns/earnings relation if X_1 represents prior-year earnings, E_t, that is:

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[18] Use of the earnings change variable constrains the coefficients on E_{t+1} and E_t to be the same in absolute amount, and opposite in sign, i.e., $\alpha_1 UE_{t+1} = \alpha_1 E_{t+1} - \alpha_1 E_t$.

$$SAR_{t+1} = \gamma_0 + \gamma_1 E_{t+1} + \gamma_2(E_{t+1} - E_t) + \nu_{t+1} \quad \text{(A1)}$$
$$= \gamma_0 + (\gamma_1 + \gamma_2)E_{t+1} - \gamma_2 E_t + \nu_{t+1}. \quad \text{(A1a)}$$

The independent variables in equations (A1) and (A1a) are a linear transformation of those in equation (5b), because $\Delta E_{t+1} = E_{t+1} - X_1$. Correspondingly, $\phi_0 = \gamma_0$, $\alpha_1 = (\gamma_1 + \gamma_2)$, and $\phi_1 = -\gamma_2$. If the ratio $-\gamma_2/(\gamma_1+\gamma_2)$ is less than 1.0, and X_1 is specified as E_t, then this implies that the cross-sectional variance of $E(E_{t+1})$ is less than the cross-sectional variance of E_t.[19]

Similar reasoning applies when we use analysts' earnings forecasts as proxies for the security market's earnings expectations. In that case, estimating equation (5b) allows the slope parameters to impound investors' implicit adjustments of the cross-sectional variance of analysts' forecasts. Previous findings that earnings levels and changes, or surprises, are incrementally significant in returns/earnings regressions (Easton and Harris 1991) are interpretable as evidence that the security market's earnings expectations impound some linear transformation of the expected earnings proxy. When the estimated slope parameters on the levels-and-changes, or surprises, variables are both positive, this implies that market expectations reflect a belief that the cross-sectional variance of the expectations proxy is too large.

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[19] $Var[(-\gamma_2/(\gamma_1 + \gamma_2))E_t] = (-\gamma_2/(\gamma_1 + \gamma_2))^2 var(E_t)$, so if γ_1 and γ_2 have the same sign, then $var[E(E_{t+1})] < var(E_t)$.

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The Relation among Disclosure, Returns, and Trading Volume Information

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ABSTRACT: This paper introduces a model in which the firm's returns depend on trading volume when the firm defers disclosure, because market makers use volume to draw inferences about better-informed investors' private information on firm value. In addition, we show that a firm's commitment to a policy of timely disclosure of a broader range of outcomes dampens the slope coefficient on volume in a Taylor expansion of the log of the absolute value of returns on volume. The reason for this is that when a firm commits to a policy of timely disclosure of a broader range of outcomes, the deferral of a report indicates that the outcome is extreme. This heightens adverse selection. In turn, this makes informed trade more costly and hence less likely, thereby rendering returns less dependent on trading volume as a source of information about firm value. This result predicts that firms committing to more disclosure should experience a smaller slope coefficient on trading volume. Thus, the slope coefficient offers a potential tool for measuring the economic consequences of shifts in disclosure regimes.

Keywords: *disclosure; trading volume information; asset pricing.*

I. INTRODUCTION

In this paper we introduce a model whereby a firm adopts a disclosure policy in which it reports current period performance in a timely fashion only when the performance falls within some range of results, and defers disclosure to a later period when performance falls outside the range. A firm may defer reporting current period performance outside some range of results because performance results outside the range may indicate

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that the results are extreme, and extreme results may require more scrutiny by, or further consultation with, top managers, audit committees, firm directors, etc., before public release. We show that when the firm defers the report, market makers use trading volume information to draw inferences about better-informed investors' private information on firm value. Thus, the firm's stock returns depend on trading volume.

To facilitate potential empirical applications of our analysis, we linearize our expression for the log of the absolute value of the firm's returns (log-returns, hereafter) by regressing log-returns on trading volume with a first-order Taylor series expansion. Our model predicts that the slope coefficient on trading volume falls as the range of performance the firm commits to report in a timely fashion expands, *whenever realized performance is such that the firm defers disclosure.* When a firm has committed to a policy of timely disclosure of a broader range of outcomes, the deferral of the report indicates that the outcome is extreme. This heightens adverse selection. Heightened adverse selection implies that market makers are at a greater informational disadvantage relative to informed investors. This informational disadvantage compels market makers to sell higher/buy lower to price-protect themselves. In turn, this makes informed trade more costly and hence less likely. Thus, trading volume reveals less about informed investors' information, thereby rendering log-returns less dependent on volume as a source of information about firm value.

Our model also predicts that the slope coefficient on trading volume falls *in expectation* (i.e., unconditionally, whether or not the firm defers disclosure) as the range of performance the firm commits to report in a timely fashion expands, provided that the slope coefficient on volume is positive. The intuition for this result is that when the firm discloses, market makers rely on the disclosure itself, rather than on alternative sources of information about firm value, such as volume. Thus, as the range of performance the firm commits to report in a timely fashion expands, log-returns are less likely to be associated with volume. This predicts that the slope coefficient on volume falls in expectation, provided that the slope is positive (if the slope is negative, less of it results in a positive, not negative, effect). In addition, as the range of performance the firm commits to report in a timely fashion expands, the slope coefficient on volume falls when the firm defers disclosure (for the reason discussed in the previous paragraph). Both these two effects predict that the slope coefficient on trading volume falls in expectation as the range of performance the firm commits to report in a timely fashion expands.

Although we couch our analysis in terms of a firm committing to a policy of timely disclosure of a broader range of outcomes, one can interpret our results as speaking to disclosure in general. Our analysis therefore posits that an event that unambiguously increases a firm's commitment to more disclosure in general should result in a corresponding decrease in the slope coefficient on trading volume in a first-order Taylor expansion of log-returns on trading volume (provided that the slope is positive). Thus, our model suggests the slope coefficient on volume as a measure of disclosure. Researchers might use this measure to assess the economic consequences of changes in disclosure regimes—that is, whether a commitment to more (or less) disclosure has measurable economic consequences—provided that the analysis controls for other determinants of return and trading volume behavior (see Core 2001).

Researchers considering using the slope coefficient on volume as a measure of disclosure in an empirical study, however, should consider two important caveats. First, our model assumes the existence of an adverse selection problem. An adverse selection problem arises when market makers execute trades with better-informed investors. In the absence of adverse selection there may be no role for volume to play as an information source. Second, we assume that trading volume is a valuable source of information about the asset's value. To the extent that other, more transparent information, such as detailed and comprehensive financial reports, analyst or management forecasts, etc., subsume volume's information about firm value, volume may not play an economically significant role in real institutional settings. In other words, the existence of other, more transparent information sources may attenuate the relation between disclosure and the slope coefficient in a regression of log-returns on trading volume. However, this is essentially an open empirical question.

The contribution of our analysis is twofold. First, prior research has overlooked the role of trading volume as an auxiliary source of information about firm value, because of difficulty in modeling its role. Our analysis represents one of the few attempts to grapple with these complexities and explicitly incorporate trading volume information into firm valuations. (For more discussion on this topic, see Verrecchia [2001].) Second, the model contributes to our understanding of the relations among disclosure, returns, and trading volume by offering an interpretation of the slope coefficient on trading volume in a Taylor expansion of log-returns on volume as a measure of disclosure.

In the next section we compare and contrast our work to prior research on trading volume. In Section III we outline and explain the assumptions underlying our model of trade. As we discuss, our model has elements and features that are common in the literature, but it is unique in the way it combines those elements. In Section IV we demonstrate the existence of an equilibrium to our model of trade. In Section V we discuss how disclosure affects adverse selection in the context of our model. In Section VI we show that the expression for log-returns that results from a first-order Taylor expansion on trading volume has the feature that the slope coefficient decreases as disclosure increases, both conditional on deferred disclosure and unconditionally (provided that the slope is positive). The concluding section briefly summarizes the results of our analysis.

II. PRIOR RESEARCH

The assumptions underlying our model integrate ideas form Glosten and Milgrom (1985) (hereafter GM) and Kyle (1985) (hereafter Kyle). As in GM, we assume that investors can trade only one unit of the asset (i.e., buy one unit or sell one unit), but, as in subsequent extensions of their work, we also allow investors to defer trade (i.e., neither buy nor sell) (see, for example, Diamond and Verrecchia [1987]). In addition, as in both GM and Kyle, we assume that a risk-neutral market maker executes all trades. Unlike GM, who assume that the market maker executes trades sequentially, or Kyle, who assumes that the market maker executes based on total net demand, we assume that the market maker executes trades based on order flow over some interval of time. In our model, order-flow information is equivalent to information on both total net demand and trading volume. This mechanism allows firm returns to depend on trading volume.

In our analysis, the market maker can infer information about firm value from trading volume. While considerable prior research analyzes trading volume as a *consequence* of trade (e.g., Admati and Pfleiderer 1988; Varian 1989; Kim and Verrecchia 1991a, 1991b, 1997; Haris and Raviv 1993; Kandel and Pearson 1995), in these models trading volume is not a *source* of information about firm value. Prior literature has typically abstracted from the informational role of volume to obtain a linear equilibrium characterization of asset price (e.g., Kyle 1985; Kim and Verrecchia 1991a). In contrast, trading volume is a nonlinear source of information about firm value because it measures the sum of the *absolute value* of individual demand orders. This, in turn, typically results in nonlinear price characterizations.

Theoretical research that allows the market to infer information from trading volume includes Blume et al. (1994), who propose a model of perfect competition in which investors condition their expectation of firm value over prior-period demand and trading volume information (as well as their own current period private information); Bernardo and Judd

(1996), who offer a similar model in which investors condition their expectations over past and current demand (through prices) and prior-period trading volume; and Campbell et al. (1993), who propose a model in which trading volume reveals changes in noninformational traders' demand for firm shares. In contrast to this prior work, we base our model on contemporaneous information about total net demand and trading volume through order-flow, and assume that market makers face an adverse selection problem when they execute trades with better-informed investors.[1]

III. A MODEL OF TRADE

Assumptions of the Model

We begin by assuming that in each period t a firm achieves some realized value (e.g., realized cash flow) that is represented by v_t. The v_t's are independent, identically distributed random variables with a uniform distribution between $-h$ and h, where $h > 0$. Let $F(v_t)$ represent the cumulative probability distribution of realizations of v_t; then $F(v_t) = (v_t + h)/2h$ for $v_t \in [-h, h]$. $F(v_t)$ is continuous and symmetrically distributed around 0; this implies that unrealized v_t's have an expected value of 0. The assumption of symmetry allows us to focus on equilibria on the positive half-real line, because equilibria on the negative half are equivalent except for appropriate changes in sign. The role of the uniform distribution is also primarily for convenience. As we discuss later, our results extend to other distributional forms that satisfy a very general property of behavior.

Let δ be some value between 0 and h.[2] We define a firm's disclosure policy as a commitment by the firm to report publicly v_t at time t whenever the realization of v_t falls in the interval $v_t \in [-\delta, \delta]$; alternatively, the firm reports v_t in period $t + 1$ whenever $v_t \notin [-\delta, \delta]$. For example, if $\delta = h$, then the firm always reports v_t at time t; if $\delta = 0$, then the firm always defers reporting v_t until period $t + 1$. In our model, the length of time associated with a period is unspecified. A period could be an hour, a day, a week, a month, or any unit of time. We also ignore discounting. Thus, when the firm defers its report of v_t until period $t + 1$, expected firm value based on the information the firm reports publicly up through period t (i.e., the v_t's up through period $t - 1$) is $\Sigma_{j=0}^{t-1} v_j + E[v_t \notin [-\delta, \delta]] + E[\Sigma_{j=t+1}^{\infty} v_t] = \Sigma_{j=0}^{t-1} v_j$. For convenience, let $\mu_{t-1} \equiv \Sigma_{j=0}^{t-1} v_j$.

Because our model assumes that the firm reports v_t no later than period $t + 1$, we say that a firm commits to a policy of timely disclosure (i.e., in period t) of a broader range of outcomes as δ increases. For example, our disclosure characterization implies that the firm makes timely reports when current firm performance falls within an interval of anticipated performance, but otherwise defers reporting until the next period. Suppose the firm expects earnings of \$10 million each period and sets δ equal to \$2 million.[3] In this context, the firm reports in a timely fashion earnings realizations between \$12 and \$8 million, but defers reporting earnings outside that range. A firm may defer reporting earnings realizations outside some anticipated interval because extreme earnings may require more scrutiny by, or further consultation with, top managers, audit committees, firm directors, etc., before public release. Clinch and Verrecchia (1997) offer theoretical support for a policy in which the firm discloses when current firm performance falls within some interval of anticipated performance and withholds otherwise. Specifically, they suggest a model in which two firms compete in a Cournot (quantity setting) product market, and show the existence of disclosure equilibria in which firms disclose realizations of firm performance when the realizations fall around the mean of the performance distribution and withhold when realizations fall in the tails: In their words, a policy in which firms disclosure "anticipated news" and withhold "dramatic news."

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[1] In models based on perfect competition (e.g., Blume et al. 1994; Bernardo and Judd 1996) there is no adverse selection problem because demand orders do not affect the price at which orders execute (because there exists a countably infinite number of traders). Adverse selection plays no role in Campbell et al. (1993) because noninformational traders' activity arises from transitory shifts in their tastes and preferences for an asset, and not from information that is superior or dissimilar to public knowledge.

[2] To ensure the existence of a unique equilibrium, we require that δ not be too large in relation to h. See the proof of Proposition 1 for a detailed discussion.

[3] Although our model assumes that expected performance each period is 0, without loss of generality this illustration assumes that expected performance is \$10 million.

Although we couch our analysis in terms of a firm committing to a policy of timely disclosure of a broader range of outcomes, for convenience we simply say that an increase in δ conforms with "more disclosure." As such, one can interpret our characterization of "more disclosure" as increased disclosure in general. For example, one can interpret "more disclosure" as a firm commitment to prepare its financial statements using: the most transparent set of accounting standards; the most transparent procedures within a particular set of standards; or listing on exchanges that attract the greatest analyst or investor following.

Returning to the specifics of our model, there are N traders in the economy, one of whom is informed and risk neutral. The informed trader observes v_t in period t and attempts to trade on this information when the firm defers its disclosure until period t + 1. Specifically, the informed trader knows v_t. When the firm defers disclosing v_t, the informed trader submits a demand order of 1 (buy), 0 (defer trade), or −1 (sell). There are also N − 1 uninformed or liquidity traders, each of whom independently submits a demand order of 1, 0, or −1. The probability that an individual liquidity trader submits a demand order of 0 is $x \in (0, 1)$, and the probability that he submits an order of either 1 or −1 is $\frac{1}{2}(1 - x)$. As with the assumption on $F(v_t)$, symmetry in liquidity-motivated trade facilitates the analysis without qualitatively changing the results. Every feature of the economy is common knowledge except the identity of the informed trader and knowledge of v_t in period t when the firm defers disclosure.

Let N_t^+ and N_t^- represent the number of 1 and −1 demand orders in period t. Let N_t^0 represent the number of traders who defer trade in period t. Each period, a risk-neutral market maker executes all demand orders after she observes the number of 1 and −1 orders. Because N is common knowledge, the market maker can also infer N_t^0 from the relation $N_t^0 = N - N_t^+ - N_t^-$. Henceforth, N_t^* represents the set of information implied by knowledge of N_t^+, N_t^-, and N_t^0; that is, $N_t^* \equiv \{N_t^+, N_t^-, N_t^0\}$.

Assumptions of the Trading Game

P_t is the price of the firm in period t. If the firm discloses v_t in a timely fashion, then $P_t = \mu_{t-1} + v_t$. On the other hand, if the firm defers disclosure of v_t until period t + 1, then P_t is determined as follows. First, the informed trader, who observes the realization of v_t, and liquidity traders, who trade for reasons unrelated to information, submit demand orders for firm shares to a market maker. Then the market maker sets the price at which trades execute, P_t, equal to the firm's expected value conditional on (1) realizations of the v_t's up through period t − 1, μ_{t-1}; (2) order flow information in period t, N_t^*; and (3) the fact that disclosure has been deferred. When the firm defers disclosure, the trading game follows these steps:

1) In period t firm value is realized; this is represented by v_t.
2) The informed trader observes v_t.
3) The risk-neutral informed and uninformed traders submit demand orders to the market maker.

4) Based on knowledge of μ_{t-1}, N_t^*, and the fact that the firm defers disclosure, a risk-neutral market maker sets the price at which trades execute in period t (i.e., she sets P_t equal to the firm's expected value conditional on μ_{t-1}, N_t^*, and $v_t \notin [-\delta, \delta]$. All trades then execute at P_t.
5) In period t + 1, the firm publicly reports v_t.

A key feature of this model is that the market maker conditions her beliefs about firm value in period t based on order flow information in that period, specifically, the actual number of demand orders of each type, $N_t^* \equiv \{N_t^+, N_t^-, N^0\}$. Let D_t and V_t represent total net demand and trading volume, respectively. In our model, order flow information is equivalent to knowledge of total net demand *and* trading volume through N_t^* and the relations $D_t = N_t^+ - N_t^-$ and $V_t = N_t^+ + N_t^-$. In other words we develop a model in which the market maker infers additional information about firm value from trading volume, and then uses this information in setting price at time t.[4]

IV. DETERMINING AN EQUILIBRIUM

Some Preliminary Analysis

In this section we determine and analyze an equilibrium to our model of trade in the event that the firm defers disclosure (i.e., $v_t \notin [-\delta, \delta]$). Deferred disclosure occurs with probability $\Pr(v_t \notin [-\delta, \delta]) = F(-\delta) + 1 - F(\delta) = 2(1 - F(\delta))$, because of symmetry. Next, consider the informed trader's problem. The informed trader submits an order without knowing the price at which his trade will execute. Let d_t represent the informed trader's demand order and P_t the price at which trades execute in period t. For convenience, we decompose P_t as follows: $P_t \equiv \mu_{t-1} + \Delta P_t$. That is, ΔP_t represents the effect on price in period t related to order flow in that period, N_t^*, and the fact that the firm defers disclosure. In contrast, ΔP_t is unrelated to past realizations of value, μ_{t-1}. The informed trader chooses d_t to maximize his expected profit based on the effect that his action has on the expected price at which trades execute:

$$\begin{aligned} d_t &= \text{Arg max } d_t\left(\sum_{j=0}^{t} v_j - E[P_t|d_t]\right) \\ &= \text{Arg max } d_t(v_t - E[\Delta P_t|d_t]). \end{aligned}$$

Let λ be some value between δ and h: that is, $\lambda \in [\delta, h]$. For each λ in this interval, we can characterize the informed trader's trading rule or strategy as follows. The informed trader chooses $d_t = 1$ for all $v_t \geq \lambda$ such that $\lambda = E[\Delta P_t|d_t = 1]$; he chooses $d_t = -1$ for all $v_t \leq -\lambda$ such that $-\lambda = E[\Delta P_t|d_t = -1]$; and he chooses $d_t = 0$ for all $v_t \in [-\lambda, -\delta] \cup [\delta, \lambda]$ (remember that if $v_t \in [-\delta, \delta]$, then the firm discloses v_t at time t). Based on this strategy, the probabilities that the informed trader submits demand orders of $d_t = 1$, $d_t = -1$, and $d_t = 0$ when the firm defers disclosure are, respectively, $(1 - F(\lambda))/2(1 - F(\delta))$; $F(-\lambda)/2(1 - F(\delta)) = (1 - F(\lambda))/2(1 - F(\delta))$; and $(F(-\delta) - F(-\lambda) + F(\lambda) - F(\delta))/2(1 - F(\delta)) = (F(\lambda) - F(\delta))/(1 - F(\delta))$, where the last two relations follow from symmetry.[5]

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[4] Trading volume played no informational role in either GM or Kyle. In GM, the market maker executes each trader's order sequentially, that is, one order at a time. Thus, volume plays no informational role because the volume associated with each order equals the absolute value of the order itself, that is, $V_t \equiv |D_t|$. In other words, in GM knowledge of total net demand subsumes trading volume information. This limitation does not arise in Kyle, where trading volume could be a useful source of additional information about firm value, because in general $V_t \neq |D_t|$. Nonetheless, Kyle assumes that firm value is based only on knowledge of total net demand (i.e., D_t), so trading volume plays no role as an information source, by assumption.

[5] λ does not depend on t because the distribution of the v_t's, $F(v_t)$, and the total (potential) number of traders, N, are both time invariant.

We restrict an equilibrium to this trading game to one that fulfills the market-maker's conjecture that there exists an $\hat{\lambda}$, $\hat{\lambda} \in [\delta, h]$, such that: when the informed trader observes a value of $v_t \geq \hat{\lambda}$, he submits a demand order of 1; when the informed trader observes a value of v_t between $-\hat{\lambda}$ and $-\delta$ or between δ and $\hat{\lambda}$, he submits an order of 0; and, finally, when the informed trader observes a value of $v_t \leq -\hat{\lambda}$, he submits an order of -1. Symmetry implies that all the results with λ are true with $-\lambda$, with appropriate changes in sign. For this reason, we prove our results for λ only.

Recall that the market maker executes demand orders at P_t. As standard in the literature, we assume that competition for market-making services induces the market marker to execute demand orders at a price equal to firm value conditional on her knowledge, that is, $P_t = E[\Sigma_{j=0}^{t} v_t | N_t^*, \mu_{t-1}, v_t \notin [-\delta, \delta]]$. A rational expectations equilibrium to this game is characterized as follows:

1) The market maker chooses $P_t = E[\Sigma_{j=0}^{t} v_t | N_t^*, \mu_{t-1}, v_t \notin [-\delta, \delta]]$ after observing N_t^* based on her conjecture that the informed trader uses $\hat{\lambda}$ in choosing his trading rule;
2) Anticipating the market-maker's behavior, the informed trader chooses a trading rule characterized by λ; and
3) In equilibrium, $\hat{\lambda} = \lambda$ (the market maker correctly anticipates the informed investor's trading rule).

In our paper the (endogenous) parameter λ characterizes an equilibrium. A conventional interpretation of λ is that it is the inverse of market depth (i.e., the inverse of the quote of the number of shares that will be exchanged at some price). In the specific context of our model, λ represents the extent to which a unit of informed demand is expected to move price in period t (i.e., $\lambda = E[\Delta P_t | d_t = 1]$). Consequently, we interpret λ as the amount the informed investor expects his trade to affect price.

Existence of an Equilibrium

When the firm defers disclosure, the market maker either knows or infers the number of 1, 0, and -1 demand orders from observing N_t^*. From these she can infer the probability that the informed trade was 1, 0, or -1. For example, the probability that $d_t = 1$ and $N_t^* = \{N_t^+, N_t^-, N_t^0\}$ is the product of the probability that the informed trader submits a demand order of 1 in a period when the firm has deferred disclosure, which is $(1 - F(\lambda))/2(1 - F(\delta))$, and the probability that the remaining $N - 1$ trades are some combination of $N_t^+ - 1$, N_t^-, and N_t^0 uninformed trades. Formally:

$$\Pr(d_t = 1 \text{ and } N_t^*) = \frac{1 - F(\lambda)}{2(1 - F(\delta))} \cdot \frac{(N - 1)!}{(N_t^+ - 1)!(N_t^-)!(N_t^0)!} x^{N_t^0} \left(\frac{1 - x}{2}\right)^{N - N_t^0 - 1} \tag{1}$$

for $N_t^+ > 0$, and $\Pr(d_t = 1 \text{ and } N_t^*) = 0$ for $N_t^+ = 0$. Similary, the probability that $d_t = 0$ and $N_t^* = \{N_t^+, N_t^-, N_t^0\}$ is the product of the probability that the informed trader submits a demand order of 0, which is $(F(\lambda) - F(\delta))/(1 - F(\delta))$, and the probability that the remaining $N - 1$ trades are some combination of N_t^+, N_t^-, and $N_t^0 - 1$ uninformed trades. Formally:

$$\Pr(d_t = 0 \text{ and } N_t^*) = \frac{F(\lambda) - F(\delta)}{1 - F(\delta)} \cdot \frac{(N - 1)!}{(N_t^+)!(N_t^-)!(N_t^0 - 1)!} x^{N_t^0 - 1} \left(\frac{1 - x}{2}\right)^{N - N_t^0} \tag{2}$$

for $N_t^0 > 0$, and $\Pr(d_t = 0 \text{ and } N_t^*) = 0$ for $N_t^0 = 0$. Finally, the probability that $d_t = -1$

and $N_t^* = \{N_t^+, N_t^-, N_t^0\}$ is the product of the probability that the informed trader submits a demand order of -1, which is $(1 - F(\lambda))/2(1 - F(\delta))$, and the probability that remaining $N - 1$ trades are some combination of N_t^+, $N_t^- - 1$, and N_t^0 uninformed trades. Formally:

$$\Pr(d_t = -1 \text{ and } N_t^*) = \frac{1 - F(\lambda)}{2(1 - F(\delta))} \cdot \frac{(N-1)!}{(N_t^+)!(N_t^- - 1)!(N_t^0)!} x^{N_t^0} \left(\frac{1-x}{2}\right)^{N - N_t^0 - 1} \tag{3}$$

for $N_t^- > 0$, and $\Pr(d_t = -1 \text{ and } N_t^*) = 0$ for $N_t^- = 0$.

Let $\Delta P_t(N_t^*)$ represent the market-maker's expectation of v_t after observing N_t^*, given a conjecture λ.[6] The expression $\Delta P_t(N_t^*)$ is equal to $E[v_t|v_t \geq \lambda]$ multiplied by the product of 1 multiplied by the relative probability that the informed trader submits a demand order of $d_t = 1$; plus 0 multiplied by the relative probability that the informed trader submits a demand order of $d_t = 0$; minus 1 multiplied by the relative probability that the informed trader submits a demand order of $d_t = -1$. Formally, the market maker sets $\Delta P_t(N_t^*)$ such that:

$$\begin{aligned}\Delta P_t(N_t^*) &= E[v_t|N_t^* \equiv \{N_t^+, N_t^-, N_t^0\}] \\ &= E[v_t|v_t \geq \lambda] \\ &\quad \times \frac{1 \cdot \Pr(d_t = 1 \,\&\, N_t^*) + 0 \cdot \Pr(d_t = 0 \,\&\, N_t^*) - 1 \cdot \Pr(d_t = -1 \,\&\, N_t^*)}{\Pr(d_t = 1 \,\&\, N_t^*) + \Pr(d_t = 0 \,\&\, N_t^*) + \Pr(d_t = -1 \,\&\, N_t^*)} \\ &= \frac{E[v_t|v_t \geq \lambda] \cdot (1 - F(\lambda))x(N_t^+ - N_t^-)}{(1 - F(\lambda))x(N_t^+ + N_t^-) + (F(\lambda) - F(\delta))(1 - x)N_t^0}.\end{aligned} \tag{4}$$

Equation (4) is true for any $N_t^* \equiv \{N_t^+, N_t^-, N_t^0\}$, and can be rearranged to yield the following expression for $\Delta P_t(N_t^*)$.

Lemma 1: The market maker's expectation of v_t conditional on $N_t^* \equiv \{N_t^+, N_t^-, N_t^0\}$ is:

$$\begin{aligned}\Delta P_t(N_t^*) &= E[v_t|v_t \geq \lambda] \cdot \left(\frac{N_t^+ - N_t^-}{N}\right) \\ &\quad \cdot \left[1 + \frac{N_t^0(F(\delta)(1 - x) - F(\lambda) + x)}{(N - N_t^0)(F(\delta)(1 - x) - F(\lambda) + x) + N(F(\lambda) - F(\delta))(1 - x)}\right].\end{aligned} \tag{5}$$

Anticipating the market-maker's behavior, the informed trader chooses $d_t = 1$ whenever submitting a buy order results in a lower expected price than v_t. The following lemma gives an expression for the expected price when informed demand is $d_t = 1$. The proofs to all subsequent results appear in the appendix.

Lemma 2: The expected price when the informed trader submits a demand order of $d_t = 1$ is:

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[6] Formally, any expression that results from the market-maker's beliefs about λ should have a caret over the λ (i.e, $\hat{\lambda}$), as $\hat{\lambda}$ represents the market-maker's conjecture about λ. To simplify the notation, however, our discussion suppresses the caret.

$$E[\Delta P_t|d_t = 1] = \left(\frac{E[v_t|v_t \geq \lambda]}{N}\right)\left[1 + \sum_{k=1}^{N}\left(\frac{(N-1)!x^{N-k}(1-x)^{k-1}}{(N-k)!(k-1)!}\right.\right.$$
$$\left.\left.\times \frac{(N-k)(F(\delta)(1-x) - F(\lambda) + x)}{k(F(\delta)(1-x) - F(\lambda) + x) + N(F(\lambda) - F(\delta))(1-x)}\right)\right]. \quad (6)$$

Lemma 2 is an important step toward characterizing an equilibrium, as given below.

The informed trader's optimal trading rule is to choose $d_t = 1$ whenever $v_t \geq E[\Delta P_t|d_t = 1]$. Therefore, the market-maker's conjecture about λ is fulfilled if and only if $\lambda = E[\Delta P_t|d_t = 1]$. Note that equation (7) below follows from the fact that $\lambda = E[\Delta P_t|d_t = 1]$ in equilibrium and Lemma 2.

Proposition 1: For an economy with one informed trader and at least one liquidity trader (i.e., $N \geq 2$), there exists a unique equilibrium that can be characterized by a (unique) λ that satisfies:

$$\frac{N\lambda}{E[v_t|v_t \geq \lambda]} - 1 = \sum_{k=1}^{N}\left(\frac{(N-1)!x^{N-k}(1-x)^{k-1}}{(N-k)!(k-1)!}\right.$$
$$\left.\times \frac{(N-k)(F(\delta)(1-x) - F(\lambda) + x)}{k(F(\delta)(1-x) - F(\lambda) + x) + N(F(\lambda) - F(\delta))(1-x)}\right). \quad (7)$$

We restrict the discussion to a single informed trader, because this considerably reduces the computational complexity without significantly changing the underlying economic intuition. In principle, however, it should be possible to generalize Proposition 1, provided that the number of informed traders does not exceed the number of liquidity traders.[7] We also restrict the discussion to the uniform distribution for realizations of v_t. A sufficient condition for the existence and uniqueness of an equilibrium, however, is simply that $\lambda / E[v_t|v_t \geq \lambda]$ be increasing in λ. Although not all distribution functions that are continuous and symmetric exhibit this property, two common symmetric distribution functions, the uniform and normal, do, as well as others that are not symmetric.[8]

V. DISCLOSURE EFFECTS

In this section we discuss two effects of more disclosure. One effect of more disclosure is that it exacerbates adverse selection when the firm defers disclosure. That is, the broader the range of outcomes the firm commits to report in a timely fashion, the more severe the adverse selection problem when the firm defers disclosure. To demonstrate this, consider how the amount the informed trader expects his trade to affect price, λ, changes as disclosure increases. For convenience, we represent the derivative of λ with respect to our disclosure parameter, δ, as λ'.

Corollary 1: Given that the firm defers disclosure, the amount the informed investor expects his trade to affect price, λ, increases as disclosure increases (i.e., as δ increases); that is $\lambda' > 0$.

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[7] If the number of informed traders exceeds the number of uninformed traders, and if informed traders behave similarly, the market maker can identify the informed order. From N_t^+, N_t^0, and N_t^- the market maker knows the number of 1, 0, and −1 orders; consequently, the majority type is the informed order. In other words, when the number of informed exceeds the number of uninformed, an equilibrium may not exist because noise (in the form of liquidity trades) is insufficient to mask the informed investor's trading strategies.

[8] A proof that the normal distribution function satisfies this property is available from the authors, as is an example of a symmetric distribution function without this property.

Corollary 1 implies that as disclosure increases, the amount the informed investor expects his trade to affect price increases when the firm defers disclosure. Thus, the informed investor faces a more severe adverse selection problem when the firm defers disclosure.

While a commitment to disclose a broader range of outcomes exacerbates adverse selection when the firm defers disclosure, such a commitment also increases the likelihood that the firm reports firm performance in a timely fashion, and when disclosure is timely there is no adverse selection. Thus, committing to disclose a broader range of outcomes creates countervailing effects. To understand how these two (countervailing) forces affect adverse selection when taken together, consider the informed investor's trading profits. When a firm defers disclosure, the informed investor gains, the market maker breaks even (conditional on her knowledge at the time she executes trades), and uninformed investors lose. The cumulative losses of uninformed traders subsidize the market maker to ensure that she breaks even executing trades with both informed and uninformed investors. Consequently, one measure of the severity of the adverse selection problem when the firm defers disclosure is the uninformed investors' collective losses; these, in turn, equal the profit of the informed investor. Given that the firm defers disclosure, the informed investor's expected profits are the probability that it is worth his while to trade when the firm defers disclosure, which is $2(1 - F(\lambda))/2(1 - F(\delta)) = (1 - F(\lambda))/(1 - F(\delta))$, times his expected profit conditional on the fact that he submits a demand order either to buy or sell one share, which is:

$$\begin{aligned}&E[d_t(v_t - \Delta P_t)|\text{either } v_t \in [-h, -\lambda] \text{ and } d_t = -1, \text{ or } v_t \in [\lambda, h] \text{ and } d_t = 1]\\ &= \left(\frac{\int_\lambda^h v_t dF(v_t)}{1 - F(\lambda)} - \lambda\right).\end{aligned}$$

Deferred disclosure, however, only occurs with probability $2(1 - F(\delta))$. Thus, the informed investor's unconditional expected profits are:

$$2(1 - F(\delta))\frac{1 - F(\lambda)}{1 - F(\delta)}\left(\frac{\int_\lambda^h v_t dF(v_t)}{1 - F(\lambda)} - \lambda\right) = 2\left(\int_\lambda^h v_t dF(v_t) - \lambda(1 - F(\lambda))\right).$$

For the uniform distribution, this expression reduces to $2(((h^2 - \lambda^2)/4h) - (\lambda((h - \lambda)/2h))) = (1/2h)(h - \lambda)^2$. Taking the derivative of this expression with respect to δ (i.e., more disclosure) yields:

$$-\frac{1}{h}(h - \lambda)\lambda'.$$

This expression is negative because $\lambda' > 0$ from Corollary 1. Thus, more disclosure reduces the informed investor's expected profits unconditionally. Hence, in our model more disclosure unconditionally ameliorates adverse selection.

VI. RETURNS AND TRADING VOLUME

An Expression for Log-Returns

We now consider the relation among disclosure, returns, and trading volume. As an extension of our analysis in Section III, we first offer an expression for the expected log of the absolute value of the firm's returns (log-returns), conditional on trading volume and the number of traders, when firms defer disclosure. Then we reinterpret this expression as a first-order Taylor series expansion on trading volume, and show that as disclosure increases, log-returns are less associated with trading volume.

To start, consider the change in price in period t when the firm defers disclosure of performance in period t, but conditional on knowledge of performance up through period t − 1. Here, the change in price is $P_t - \mu_{t-1} = \Delta P_t(N_t^*)$, so log-returns are $\log |\Delta P_t(N_t^*)/\mu_{t-1}|$. Recall that our model defines trading volume as $V_t \equiv N_t^+ + N_t^-$. The following lemma establishes a relation among log-returns, trading volume, and the number of informed traders when trading volume assumes odd values. As the Appendix explains in greater detail, we restrict the following result to odd-valued V_t's and N's because when V_t is even it allows $\Delta P_t(N_t^*)$ to be 0 and the log of 0 is undefined. While there are a variety of ways to address this probelm, for convenience we assume that both V_t and N are odd-valued.[9]

Lemma 3: When the firm defers disclosure of current performance but all market participants know the firm's performance in previous periods, the expected log of the absolute value of the firm's returns (log-returns) in period t conditional on total trading volume, V_t, and the number of traders, N, is:

$$E_{N_t^*}\left[\log\left|\frac{\Delta P_t(N_t^*)}{\mu_{t-1}}\right| \,\middle|\, V_t, N\right]$$
$$= \log\left[\frac{E[v_t|v_t \geq \lambda]}{N\mu_{t-1}}\right] + \frac{1}{2^{V_t-1}}\sum_{\theta=1}^{(v_t+1)/2}\frac{V_t!}{(V_t+1-\theta)!(\theta-1)!}\log[V_t+2-2\theta]$$
$$+ \log\left[\frac{Nx(1-F(\lambda))}{V_t(F(\lambda)(1-x)-F(\lambda)+x)+N(F(\lambda)-F(\delta))(1-x)}\right], \tag{8}$$

where V_t takes odd values of $V_t = 1,\ldots,N$, and N is odd.

In Lemma 3 and throughout this discussion, we refer to N as the "number of traders," although, more precisely, N is the *potential* number of traders because some traders may defer trade.

Disclosure, Log-Returns, and Trading Volume

Having established an expression for log-returns in Lemma 3, next we establish a relation between disclosure, δ, and trading volume through the behavior of log-returns. To achieve this, we appeal to a Taylor series expansion of log-returns as a function of trading volume in period t, V_t. Specifically, holding the number of traders, N, constant, let T(·) represent the expression for log-returns given in Lemma 3 as a function of trading volume; that is, let $T(V_t)$ be defined by:

$$T(V_t) \equiv E_{N_t^*}\left[\log\left|\frac{\Delta P_t(N_t^*)}{\mu_{t-1}}\right| \,\middle|\, V_t, N\right].$$

Taylor's formula with remainder is:

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[9] For example, an alternative approach would be to set the log of 0 to some arbitrarily large, fixed negative number.

$$T(V_t) = T(V_0) + T'(V_0)(V_t - V_0) + \cdots + \frac{1}{(k-1)!} T^{(k-1)}(V_0)(V_t - V_0)^{k-1} + R_k(V_t),$$

where V_0 is some arbitrary level of volume, $T^{(k-1)}$ represents the $k - 1$th derivative of $T(\cdot)$ with respect to V_t, $R_k(V_t) = (1/k!)\, T^{(k)}(V_0 + sV_t)V_t$, and $s \in (0, 1)$. One interpretation of a Taylor expansion is that $T(V_0)$ is the intercept term and $T^{(k-1)}(V_0)$ is the slope coefficient on $(V_t - V_0)^{k-1}$ that results from regressing log-returns on trading volume in period t.[10]

Now consider specifically $T'(V_0)$, the slope coefficient on $V_t - V_0$ in a first-order Taylor expansion. To offer some intuition as to how the nature of the equilibrium affects the slope coefficient, define the expression Ω as:

$$\Omega \equiv 1 - x - \frac{1 - F(\lambda)}{1 - F(\delta)}.$$

The slope coefficient, $T'(V_0)$, is more (less) likely positive when Ω is positive (negative).[11] The intuition underlying this claim is that Ω represents the difference in the fixed probability of an uninformed trade when the firm defers disclosure, which is $\frac{1}{2}(1 - x) + \frac{1}{2}(1 - x) = 1 - x$, vs. the endogenous probability of an informed trade when the firm defers disclosure, which is $(1 - F(\lambda) + F(-\lambda))/(1 - F(\delta) + F(-\delta)) = 2(1 - F(\lambda))/2(1 - F(\delta)) = 1 - F(\lambda)/(1 - F(\delta))$. In other words, positive Ω implies that the marginal trade is more likely uninformed; hence we refer to positive Ω as a low information asymmetry environment. Conversely, negative Ω implies that the marginal trade is more likely informed; hence we refer to negative Ω as a high information asymmetry environment. Uninformed traders elect to trade or defer trade based on a random shock. In contrast, the informed trader makes this decision based on the amount he expects his demand order to move the price at which the market maker executes his trade. The price at which the market maker executes the informed investor's trade is the cost to the informed investor of having his trade executed. Thus, when information asymmetry is low, the informed trader almost surely trades because the expected cost of his trade is low. Hence, in a low information asymmetry environment, the effect of volume on log-returns through the slope coefficient, $T'(V_0)$, is more positive because volume almost surely incorporates informed demand. Alternatively, in a high information asymmetry environment, the informed trader is unlikely to trade because the expected cost of his trade is high. Here the effect of volume on log-returns through the slope is less positive, and perhaps even negative.

Our analysis predicts that the slope coefficient $T'(V_0)$ decreases as δ increases, *ceteris paribus*. As δ increases, $T'(V_0)$ becomes less positive or more negative, or moves from being positive to negative. The intuition is that market participants' knowledge that the firm has committed to disclose a broader range of outcomes exacerbates adverse selection when the firm defers disclosure, because disclosure deferral implies that the earnings realization is too extreme. This makes informed trade less likely. But this in turn makes log-returns less dependent on trading volume as a source of information about firm value.[12] Stated somewhat differently, committing to timely disclosure of a broader range of outcomes makes informed trades more costly when the firm decides to defer disclosure, because this evidence that the earnings realization must be extreme heightens adverse selection.

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[10] While in theory V_0 can be any value, our discussion is couched in terms of the relation between log-returns and trading volume when the firm defers disclosure. This suggests that one candidate V_0 is some representative statistic, such as the mean or median, for trading volume in nonannouncement periods.

[11] The sign of the slope coefficient does not depend conclusively on the sign of Ω because other factors in the expression for $T'(V_0)$ are unrelated to those captured in Ω; $T'(V_0)$ is only more (less) likely positive when Ω is positive (negative). A proof of this claim is available from the authors.

[12] The notion of "less dependent" is straightforward when the slope coefficient is positive; it implies a flatter slope. When the slope is negative, however, "less dependent" should be interpreted as a steeper, negative slope.

Corollary 2: As disclosure increases (i.e., as δ increases), the slope coefficient on trading volume in a first-order Taylor series expansion of the expected log of the absolute value of the firm's returns (log-returns) decreases monotonically when firms defer disclosure.

The mathematical argument that underlies the proof to Corollary 2 is that as δ increases, the probability that the firm defers disclosure, $2(1 - F(\delta))$, shrinks *faster* than the probability that the informed investor trades in such a situation, which is $2(1 - F(\lambda))$. Consequently, Ω falls as δ increases. Falling Ω, however, implies that the information asymmetry environment becomes more severe when the firm defers disclosure, which makes informed trade less likely. In short, our analysis predicts that more disclosure in a timely fashion results in a decrease in the slope coefficient on trading volume (specifically, $V_t - V_0$) in a Taylor expansion of log-returns on volume in those cases where firms defer disclosure.

With regard to the unconditional relation between disclosure and the slope, note that when the firm discloses v_t, the slope coefficient on volume is 0 because there is no need to rely on alternative sources of information about firm value, such as volume. Therefore, it would seem that more disclosure reduces unconditionally the slope coefficient on volume. More disclosure makes it less likely that the market relies on trading volume information, while simultaneously leading to a decline in the slope in the event that firms defer disclosure. This reasoning is true, however, only if the slope is positive—if the slope is negative, then less of it results in a positive (not negative) effect. In other words, the relation between disclosure and the slope coefficient is unambiguous only when the slope is positive.

Corollary 3: As disclosure increases (i.e., as δ increases), the slope coefficient on trading volume in a first-order Taylor series expansion of the expected log of the absolute value of the firm's returns (log-returns) decreases monotonically and unconditionally, provided that the slope is positive.

Taken together, Corollaries 2 and 3 suggest that more disclosure reduces the slope coefficient on trading volume in a first-order Taylor series expansion, both conditional on deferred disclosure and unconditionally, provided that the slope is positive.

With regard to potential empirical applications of our model, assume that there exists some measure of disclosure variability, either among firms (i.e., cross-sectionally) or across time (i.e., intertemporally). Corollary 3 suggests that as disclosure increases, the slope coefficient on trading volume declines unconditionally, provided that the slope is positive. Therefore, provided that one can show that log-returns and trading volume are positively associated, one potential empirical application of our model is to investigate whether more (less) disclosure is associated with a decline (increase) in the slope coefficient on trading volume in a first-order Taylor series expansion of log-returns. Empirical evidence buttressing the assumption that log-returns and trading volume are positively associated includes Bamber and Cheon (1995) and Bhattacharya (2001), who report evidence that supports a positive relation between the magnitude of returns and trading volume around earnings announcements, and Karpoff (1987, Table 1), who summarizes finance research showing that trading volume is positively associated with the magnitude of returns in general (i.e., not just around announcement periods). One caveat, however, is that our analysis suggests that the slope is more likely to be positive when information asymmetry is low (i.e., $\Omega > 0$). Low information asymmetry, however, may result if there exist more transparent information sources about firm value than volume, which would attenuate the dependency of log-returns on volume. In other words, information asymmetry environments most consistent with positive slopes may also be those where trading volume information may have the least economic significance because of competing information outside the scope of our model.

An alternative empirical application is to relate disclosure variability to slopes conditional on deferred disclosure. Corollary 2 suggests that as disclosure increases, the slope coefficient on volume declines when the firm defers disclosure. A literal interpretation of our model, however, requires a disclosure event with three salient features: (1) disclosure always occurs (e.g., disclosure is mandatory); (2) disclosure timing is variable; and (3) disclosure delays are associated with large deviations from whatever disclosure was anticipated. Annual and quarterly earnings announcements meet the first two requirements because both are mandatory and occur with some timing variability. Nonetheless, Corollary 2 also requires reporting delays to be associated with large deviations from anticipated earnings announcements. As for empirical evidence on the extent to which late earnings announcements are "surprising," a number of studies report that late earnings announcements tend to be bad news surprises (e.g., Kross 1981; Atiase et al. 1989; Begley and Fischer 1998). Our model, however, requires that late earnings announcements are equally likely to be either bad *or good* news surprises, and there is little evidence of the latter.[13] This leaves open the question as to whether it is appropriate to apply our model to circumstances in which late earnings announcements are exclusively bad news surprises. The economic intuition underlying our analysis is that the adverse selection problem becomes more severe when the firm defers disclosure; this intuition may extend to circumstances in which late earnings announcements are exclusively bad news surprises.

In short, while Corollaries 2 and 3 both suggest possible empirical applications, neither is immune from caveats and economic subtleties that may limit their usefulness in empirical studies.

VII. CONCLUSION

In this paper we suggest a model of trading activity in which a firm's returns depend on trading volume in those cases where the firm defers disclosure. Returns depend on trading volume because, in lieu of the disclosure, market makers use trading volume information to draw inferences about better-informed investors' private information on firm value. In addition, we represent our expression for the log of the absolute value of the firm's returns (log-returns) in terms of trading volume with a first-order Taylor expansion. We show that the slope coefficient on volume falls as the firm commits to a broader disclosure policy, both conditional on the firm deferring disclosure and unconditionally provided that the slope coefficient is positive. The intuition for this result is that committing to a policy of increased disclosure makes informed trade more costly in those cases when the firm defers disclosure, thereby reducing the dependency of log-returns on information related to the activities of informed investors as inferred from trading volume. This result predicts that an increase (decrease) in a firm's commitment to disclose is associated with a reduction (increase) in the slope coefficient on trading volume, provided that the slope is positive. Our model suggests that the slope coefficient on trading volume may offer a tool for assessing the economic consequences of shifts in disclosure regimes.

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[13] For example, Begley and Fischer (1998, 360) conclude: ". . . for good news announcements, there appears to be no significant relation between the magnitude of the news and the announcement timing."

Our model assumes that an adverse selection problem exists and that trading volume is a valuable source of information about firm value. While adverse selection is a common feature of many market settings, more transparent information sources other than trading volume typically exist in real institutional settings. Empirical applications of our model should control for these other (unmodeled) sources of information.

In our view, the most important contribution of our work is that it adds to our understanding of how trading volume information affects firm valuations in conjunction with information about total net demand—an issue that prior research has largely overlooked because of the complexity of modeling how expectations of firm value incorporate market participants' private information as revealed through their trading activity.

APPENDIX

Proof of Lemma 2

Define $V_t \equiv N_t^+ + N_t^-$ and $D_t \equiv N_t^+ - N_t^-$. this implies that $N_t^+ = (V_t + D_t)/2$, $N_t^- = (V_t - D_t)/2$, and $N_t^0 = N - V_t$. In addition, it allows the expected ΔP_t given $d_t = 1$ to be rewritten as:

$$E[\Delta P_t | d_t = 1]$$

$$= \sum_{V_t=1}^{N} \sum_{D_t} \left[\Delta P_t \left(\left\{ \frac{V_t + D_t}{2}, \frac{V_t - D_t}{2}, N - V_t \right\} \times \frac{(N-1)! x^{N-V_t} \left(\frac{1-x}{2} \right)^{V_t - 1}}{\left(\frac{V_t + D_t}{2} - 1 \right)! (N - V_t)! \left(\frac{V_t - D_t}{2} \right)!} \right. \right] .$$

$$= \frac{E[v_t | v_t \geq \lambda]}{N} \sum_{V_t=1}^{N}$$

$$\times \sum_{D_t} \left[\left(1 + \frac{(N - V_t)(F(\delta)(1-x) - F(\lambda) + x)}{V_t(F(\delta)(1-x) - F(\lambda) + x) + N(F(\lambda) - F(\delta))(1-x)} \right) \right.$$

$$\left. \times \frac{D_t (N-1)! x^{N-V_t} \left(\frac{1-x}{2} \right)^{V_t - 1}}{\left(\frac{V_t + D_t}{2} - 1 \right)! (N - V_t)! \left(\frac{V_t - D_t}{2} \right)!} \right],$$

where D_t can take the value of V_t, $V_t - 2$, $V_t - 4, \ldots, -(V_t - 2)$. Using a different index θ_t defined by $D_t \equiv V_t + 2 - 2\theta_t$, the above can be written as:

$$
\begin{aligned}
&E[\Delta P_t|d_t = 1]\\
&= \left(\frac{E[v_t|v_t \geq \lambda]}{N}\right) \sum_{V_t=1}^{N} \left[\left(1 + \frac{(N - V_t)(F(\delta)(1 - x) - F(\lambda) + x)}{V_t(F(\delta)(1 - x) - F(\lambda) + x) + N(F(\lambda) - F(\delta))(1 - x)}\right)\right.\\
&\quad \left.\times \sum_{\theta_t=1}^{V_t} \frac{(N - 1)!(V_t + 2 - 2\theta_t)x^{N-V_t}\left(\frac{1 - x}{2}\right)^{V_t-1}}{(N - V_t)!(V_t - \theta_t)!(\theta_t - 1)!}\right]\\
&= \left(\frac{E[v_t|v_t \geq \lambda]}{N}\right)\\
&\quad \times \sum_{V_t=1}^{N} \left[\left(1 + \frac{(N - V_t)(F(\delta)(1 - x) - F(\lambda) + x)}{V_t(F(\delta)(1 - x) - F(\lambda) + x) + N(F(\lambda) - F(\delta))(1 - x)}\right)\right.\\
&\quad \left.\times \sum_{\theta_t=1}^{V_t} \frac{(N - 1)!x^{N-V_t}\left(\frac{1 - x}{2}\right)^{V_t-1}}{(N - V_t)!(V_t - \theta_t)!(\theta_t - 1)!}\right],
\end{aligned}
$$

where the second equality holds because $\Sigma_{\theta_t=1}^{V_t} (V_t + 2 - 2\theta_t)/(V_t - \theta_t)!(\theta_t - 1)! = \Sigma_{\theta_t=1}^{V_t} 1/(V_t - \theta_t)!(\theta_t - 1)!$. In order to prove this relation, note first that $(V_t - \theta_t)!(\theta_t - 1)! = (V_t - (V_t + 1 - \theta_t))!((V_t + 1 - \theta_t) - 1)! = (\theta_t - 1)!(V_t - \theta_t)!$ and that $(V_t + 2 - 2\theta_t) + (V_t + 2 - 2(V_t + 1 - \theta_t)) = 2 = 1 + 1$. If V_t is even, then any θ_t from 1 to $V_t/2$ (the first half of the V_t terms) can be matched with the corresponding $V_t + 1 - \theta_t$, i.e., V_t, $V_t - 1,\ldots,(V_t/2) + 1$ (the remaining half), and $\Sigma_{\theta_t=1}^{V_t} (V_t + 2 - 2\theta_t)/(V_t - \theta_t)!(\theta_t - 1)! = \Sigma_{\theta_t=1}^{V_t} 1/(V_t - \theta_t)!(\theta_t - 1)!$ follows. If V_t is odd, then any θ_t from 1 to $(V_t - 1)/2$ can be matched with the corresponding $V_t + 1 - \theta_t$, i.e., V_t, $V_t - 1,\ldots,(V_t + 3)/2$, as before. There is one remaining term for $\theta_t = (V_t + 1)/2$, but its numerator is also 1 and the relation still holds in this case. Equation (6) now obtains because $\Sigma_{V_t=1}^{N} \Sigma_{\theta_t=1}^{V_t} (N - 1)!x^{N-V_t}((1 - x)/2)^{V_t=1}/(N - V_t)!(V_t - \theta_t)!(\theta_t - 1)! = (x + ((1 - x)/2) + ((1 - x)/2))^{N-1} = 1$ and $\Sigma_{\theta_t=1}^{V_t} (V_t - 1)!/(V_t - \theta_t)!(\theta_t - 1)! = 2^{V_t=1}$, where we can substitute in k for V_t because in these expressions V_t is an arbitrary counter. Q.E.D.

Proof of Proposition 1

The characterization of the equilibrium follows from the characterization of $E[\Delta P|d_t = 1]$ offered in Lemma 2, setting $\lambda = E[\Delta P|d_t = 1]$, and rearranging terms. With regard to the existence of a unique equilibrium for this expression, note first that $F(\lambda) = (\lambda + h)/2h$, and thus $1 - F(\lambda) = (h - \lambda)/2h$ and $E[v_t|v_t \geq \lambda] = 1/(1 - F(\lambda)) \int_{\lambda}^{h}(1/2h)v_t dv_t = (2h/(h - \lambda))((h^2 - \lambda^2)/4h) = (h + \lambda)/2$. In addition, $F(\delta)(1 - x) - F(\lambda) + x = ((h + \delta)(1 - x)/2h) - ((h + \lambda)/2h) + x = (1/2h)(hx + \delta(1 - x) - \lambda)$. Now define $\Phi(\lambda)$ such that:

$$\Phi(\lambda) = \frac{2N\lambda}{h + \lambda} - 1 - \sum_{V=1}^{N} \left(\frac{(N-1)!(1-x)^{V-1}x^{N-V}}{(N-V)!(V-1)!} \times \frac{(N-V)(hx + \delta(1-x) - \lambda)}{V(hx + \delta(1-x) - \lambda) + N(\lambda - \delta)(1-x)} \right).$$

Then the equilibrium condition as expressed in equation (7) can be reexpressed as one of finding a $\lambda^* \in [\delta, h]$ such that $\Phi(\lambda^*) = 0$. To show this, note three facts. First:

$$\frac{\partial}{\partial\lambda}\Phi(\lambda) = \frac{2Nh}{(h+\lambda)^2} + \sum_{V=1}^{N} \left(\frac{(N-1)!(1-x)^{V-1}x^{N-V}}{(N-V)!(V-1)!} \times \frac{(N-V)N(h-\delta)(1-x)x}{[V(hx + \delta(1-x) - \lambda) + N(\lambda - \delta)(1-x)]^2} \right),$$

which is positive; this implies that $\Phi(\lambda)$ is monotonically increasing in λ. Second, for any λ and δ:

$$\lim_{\lambda \to h} \Phi(\lambda) = N - 1 + \sum_{V=1}^{N} \frac{(N-1)!(1-x)^{V-1}x^{N-V}}{(N-V)!(V-1)!} > 0.$$

Finally, when λ assumes its lowest possible value, $\lambda = \delta$:

$$\begin{aligned}\Phi(\lambda = \delta) &= \frac{2N\delta}{h+\delta} - 1 - \sum_{V=1}^{N} \left(\frac{(N-1)!(1-x)^{V-1}x^{N-V}}{(N-V)!(V-1)!} \times \frac{N-V}{V} \right) \\ &= \frac{2N\delta}{h+8} - 1 - \frac{x}{1-x} \cdot \sum_{V=1}^{N} \frac{(N-1)!(1-x)^{V}x^{N-V-1}}{(N-V-1)!V!} \\ &= \frac{2N\delta}{h+\delta} - \frac{1-x^N}{1-x},\end{aligned}$$

and this value is negative for δ/h sufficiently small: Specfically, it is negative for any δ such that $\delta/(h + \delta) < (1 - x^N)/2N(1 - x)$. Taken together, these three facts imply that there exists a unique λ^*, such that $\Phi(\lambda^*) = 0$. Q.E.D.

Proof of Corollary 1

Recall that the equilibrium condition is given by:

$$\frac{2N\lambda}{h+\lambda} - 1 - \sum_{k=1}^{N} \left(\frac{(N-1)!(1-x)^{k-1}x^{N-k}}{(N-k)!(k-1)!} \frac{(N-k)(hx + \delta(1-x) - \lambda)}{k(hx + \delta(1-x) - \lambda) + N(\lambda - \delta)(1-x)} \right)$$

equals 0. Taking the derivative of this expression with respect to δ and defining λ' as $\lambda' \equiv (\partial/\partial\delta)\,\lambda$ yields:

$$\lambda' \frac{2Nh}{(h+\lambda)^2} + \sum_{k=1}^{N} \left(\frac{(N-1)!(1-x)^{k-1}x^{N-k}}{(N-k)!(k-1)!} \frac{(N-k)N(\lambda'(h-\delta) - h + \lambda)x(1-x)}{(k(hx + \delta(1-x) - \lambda) + N(\lambda - \delta)\,(1-x))^2} \right)$$

equals 0. This implies that λ' equals:

$$\lambda' = \frac{\displaystyle\sum_{k=1}^{N}\left(\frac{(N-1)!(1-x)^{k-1}x^{N-k}}{(N-k)!(k-1)!}\,\frac{(N-k)N(h-\lambda)x(1-x)}{(k(hx+\delta(1-x)-\lambda)+N(\lambda-\delta)(1-x))^2}\right)}{\displaystyle\frac{2Nh}{(h+\lambda)^2}+\sum_{k=1}^{N}\left(\frac{(N-1)!(1-x)^{k-1}x^{N-k}}{(N-k)!(k-1)!}\,\frac{(N-k)N(h-\delta)x(1-x)}{(k(hx+\delta(1-x)-\lambda)-N(\lambda-\delta)\,(1-x))^2}\right)} > 0.$$

This completes the proof of Corollary 1. Q.E.D.

Proof of Lemma 3

Recall that Lemma 1 offers an expression for $\Delta P_t(N_t^*)$:

$$\begin{aligned}\Delta P_t(N_t^*) &= E[v_t|v_t \geq \lambda] \times \left(\frac{N_t^+ - N_t^-}{N}\right)\\ &\quad\times\left[1+\frac{N_t^0(F(\delta)(1-x)-F(\lambda)+x)}{(N-N_t^0)(F(\delta)(1-x)-F(\lambda)+x)+N(F(\lambda)-F(\delta))(1-x)}\right]\\ &= E[v_t|v_t \geq \lambda] \times \left(\frac{N_t^+ - N_t^-}{N}\right)\\ &\quad\times\left[\frac{Nx(1-F(\lambda))}{(N-N_t^0)(F(\delta)(1-x)-F(\lambda)+x)+N(F(\lambda)-F(\delta))(1-x)}\right].\end{aligned}$$

Employing this characterization, next we derive an expression for $\log|\Delta P_t(N_t^*)/\mu_{t-1}|$ in terms of trading volume and the number of traders. Note that in the context of our model, trading volume is defined by $V_t \equiv N_t^+ + N_t^- = N - N_t^0$. To obtain an expression for the log of the absolute value price change in terms of $\{V_t, N\}$, we take the expection of $\log|\Delta P_t(N_t^*)/\mu_{t-1}|$ conditional on V_t and N:

$$\begin{aligned}&E_{N_t^+}\left[\log\left|\frac{\Delta P_t(N_t^*)}{\mu_{t-1}}\right|\,\middle|\,V_t, N\right]\\ &= \log\left[\frac{E[v_t|v_t \geq \lambda]}{N\mu_{t-1}}\right] + E_{N_t^+}[\log|N_t^+ - N_t^-|\,|V_t, N]\\ &\quad+\log\left[\frac{Nx(1-F(\lambda))}{V_t(F(\delta)(1-x)-F(\lambda)+x)+N(F(\lambda)-F(\delta))(1-x)}\right].\end{aligned}$$

Now consider the expression $E_{N_t^+}[\log|N_t^+ - N_t^-|\,|V_t, N]$. Below, in a separate result, we show that $E_{N_t^+}[\log|N_t^+ - N_t^-|\,|V_t, N]$ can be expressed as:

$$E_{N_t^+}[\log|N_t^+ - N_t^-|\,|V_t, N] = \frac{1}{2^{V_t-1}}\sum_{\theta=1}^{\Theta_t}\frac{V_t!}{(V_t+1-\theta)!(\theta-1)!}\log[V_t+2-2\theta],$$

where $\Theta_t = (V_t + 1)/2$ when V_t is odd and $\Theta_t = (V_t/2) + 1$ when V_t is even. The problem with the expectation of $\log|N_t^+ - N_t^-|$ when V_t is even is that it allows for the possibility that net demand is zero (i.e., $N_t^+ - N_t^- = 0$) and the log of 0 is undefined. Consequently, for convenience we assume that both V_t and N are odd (which implies $N_t^+ - N_t^- \neq 0$).

Calculation of $E_{N_t^+}[\log|N_t^+ - N_t^-|\,|V_t]$:

To simplify the notation, let $D_t = N_t^+ - N_t^-$. Then:

$$E_{N_t^{\pm}}[\log |N_t^+ - N_t^-|\ |V_t] = E[\log |D_t|\ |V_t] = \frac{\Sigma_{D_t} (\Pr(D_t, V_t) \cdot \log [|D_t|])}{\Sigma_{D_t} \Pr(D_t, V_t)}$$

Also recall that $V_t = N_t^+ + N_t^-$. Thus, $N_t^+ = (V_t + D_t)/2$, $N_t^- = (V_t - D_t)/2$, and $N_t^0 = N - V_t$. Hence:

$$\begin{aligned}
\Pr(D_t, V_t) &= (1 - F(\lambda)) \cdot \frac{(N-1)!}{\left(\frac{V_t + D_t}{2} - 1\right)!(N - V_t)!\left(\frac{V_t - D_t}{2}\right)!} x^{N-V_t} \left(\frac{1-x}{2}\right)^{V_t - 1} \\
&\quad + 2(F(\lambda) - F(\delta)) \cdot \frac{(N-1)!}{\left(\frac{V_t + D_t}{2}\right)!(N - V_t - 1)!\left(\frac{V_t - D_t}{2}\right)!} x^{N-V_t-1} \left(\frac{1-x}{2}\right)^{V_t} \\
&\quad + (1 - F(\lambda)) \cdot \frac{(N-1)!}{\left(\frac{V_t + D_t}{2}\right)!(N - V_t)!\left(\frac{V_t - D_t}{2} - 1\right)!} x^{N-V_t} \left(\frac{1-x}{2}\right)^{V_t - 1} x^{N-V_t} \\
&= \frac{(N-1)!x^{N-V_t-1}\left(\frac{1-x}{2}\right)^{V_t-1}}{\left(\frac{V_t + D_t}{2}\right)!(N - V_t)!\left(\frac{V_t - D_t}{2}\right)!} (V_t(F(\delta)(1 - x) - F(\lambda) + x) \\
&\quad + N(F(\lambda) - F(\delta))(1 - x)).
\end{aligned}$$

We now use the fact that anything in the expression for $\Pr(D_t, V_t)$ that does not involve D_t cancels out in the numerator and denominator in the expression for $E_{N_t^{\pm}}[\log |D_t|\ |V_t]$ above, and the fact that:

$$\sum_{D_t} \frac{V_t!}{\left(\frac{V_t + D_t}{2}\right)!\left(\frac{V_t - D_t}{2}\right)!} = 2^{V_t},$$

to obtain:

$$\begin{aligned}
E_{N_t^{\pm}}[\log |D_t|\ |V_t] &= \frac{\displaystyle\sum_{D_t} \frac{V_t!}{\left(\frac{V_t + D_t}{2}\right)!\left(\frac{V_t - D_t}{2}\right)!} \log |D_t|}{2^{V_t}} \\
&= \sum_{D_t} \frac{V_t!\left(\frac{1}{2}\right)^{V_t}}{\left(\frac{V_t + D_t}{2}\right)!\left(\frac{V_t - D_t}{2}\right)!} \log |D_t|.
\end{aligned}$$

We now use a new index, $\theta \equiv ((V_t - D_t)/2) + 1$. When V_t is odd, the above is further rewritten as:

$$E_{N_t^*}[\log |D_t|\ |V_t] = 2 \sum_{\theta=1}^{(V_t+1)/2} \frac{V_t!\left(\frac{1}{2}\right)^{V_t}}{(V_t + 1 - \theta)!(\theta - 1)!} \log [V_t + 2 - 2\theta]$$

$$= \frac{1}{2^{V_t-1}} \sum_{\theta=1}^{(V_t+1)/2} \frac{V_t!}{(V_t + 1 - \theta)!(\theta - 1)!} \log [V_t + 2 - 2\theta],$$

where the first equality obtains by symmetry. Similarly, when V_t is even, it is:

$$E_{N_t^*}[\log |D_t|\ |V_t] = 2 \sum_{\theta=1}^{(V_t/2)+1} \frac{V_t!\left(\frac{1}{2}\right)^{V_t}}{(V_t + 1 - \theta)!(\theta - 1)!} \log [V_t + 2 - 2\theta]$$

$$= \frac{1}{2^{V_t-1}} \sum_{\theta=1}^{(V_t/2)+1} \frac{V_t!}{(V_t + 1 - \theta)!(\theta - 1)!} \log [V_t + 2 - 2\theta].$$

This completes the proof of Lemma 3. Q.E.D.

Proof of Corollary 2

Recall that for the uniform distribution, $F(\lambda) = (h + \lambda)/2h$, $1 - F(\lambda) = (h - \lambda)/2h$, $F(\delta)(1 - x) - F(\lambda) + x = (1/2h)(hx + \delta(1 - x) - \lambda)$ and $E[v_t|v_t \geq \lambda] = (h + \lambda)/2$. This implies that the expression for $E_{N_t^*}[\log |\Delta P_t(N_t^*)/\mu_{t-1}|\ |V_t, N]$ offered in Lemma 3 can be expressed as:

$$E_{N_t^*}\left[\log \left|\frac{\Delta P_t(N_t^*)}{\mu_{t-1}}\right| \ \middle|\ V_t, N\right]$$
$$= \log \left[\frac{h + \lambda}{2N\mu_{t-1}}\right] + \frac{1}{2^{V_t-1}} \sum_{\theta=1}^{(V_t+1)/2} \frac{V_t!}{(V_t + 1 - \theta)!(\theta - 1)!} \log [V_t + 2 - 2\theta]$$
$$+ \log \left[\frac{Nx(h - \lambda)}{V_t(hx + \delta(1 - x) - \lambda) + N(\lambda - \delta)(1 - x)}\right]. \qquad \text{(A1)}$$

Now note that the first expression on the right-hand side of equation (A1) is not a function of V_t and hence can be ignored. In effect, it is part of the intercept term $T(V_0)$. The second expression is a function of V_t and hence is a part of $T'(\cdot)$. It is not, however, a function of λ or δ and hence is invariant with respect to changes in δ. Therefore, it too can be ignored. The derivative of the third expression with respect to V_t is:

$$\frac{\partial}{\partial V_t} \log \left[\frac{Nx(h - \lambda)}{V_t(hx + \delta(1 - x) - \lambda) + N(\lambda - \delta)(1 - x)}\right]$$
$$= -\frac{hx + \delta(1 - x) - \lambda}{V_t(hx + \delta(1 - x) - \lambda) + N(\lambda - \delta)(1 - x)}. \qquad \text{(A2)}$$

Define λ' as $\lambda' \equiv (\partial/\partial\delta)\lambda$, and note that the derivative of equation (A2) with respect to δ yields:

$$\frac{\partial}{\partial\delta}\left[\frac{hx + \delta(1 - x) - \lambda}{V_t(hx + \delta(1 - x) - \lambda) + N(\lambda - \delta)(1 - x)}\right]$$
$$= \frac{N(\lambda'(h - \delta) - (h - \lambda))x(1 - x)}{(V_t(hx + \delta(1 - x) - \lambda) + N(\lambda - \delta)(1 - x))^2}. \tag{A3}$$

Finally, substituting the expression λ' (derived in Corollary 1) into equation (A3) yields an expression that is proportional to:

$$-\frac{2N^2hx(1 - x)}{(h + \lambda)^2}(h - \lambda) < 0.$$

This proves our claim that $T'(V_0)$ decreases as δ increases. Q.E.D.

Proof of Corollary 3

Let β represent the slope coefficient on trading volume when disclosure is deferred. When disclosure is timely, log-returns do not depend on trading volume information; hence the slope is 0. Consequently, the unconditional slope coefficient is $2(1 - F(\delta))\beta = ((h - \delta)/h)\beta$. The derivative of this expression with respect to more disclosure, δ, is:

$$-\frac{1}{h}\beta + \left(\frac{h - \delta}{h}\right)'\beta', \tag{A5}$$

where β' represents the derivative of β with respect to δ. If β is positive, then the first expression in equation (A5) (i.e., $-(1/h)\beta$) is negative. From Corollary 2, we know that the derivative of the slope, β', is always negative, and hence the second expression (i.e., $((h - \delta)/h)\beta'$) is always negative. The proves our claim that the slope decreases unconditionally as δ increases, provided that the slope is positive. Q.E.D.

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Taxes as a Determinant of Managerial Compensation in Privately Held Insurance Companies

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ABSTRACT: This study empirically investigates how taxes affect managerial compensation for a sample of privately held insurers whose managers own a large percentage of the firm's stock (I refer to these as management-owned insurers) during 1989–1996. Shareholder/managers receive two types of income from the firm they own: compensation income as employees, and investment income as shareholders. Although compensation income is taxable to employees and deductible by employers, investment income is subject to double taxation. Thus, the mix of the two is an important tax-planning decision for management-owned insurers. I predict and find that as individual tax rates increased relative to corporate tax rates from 1989–1992 to 1993–1996, shareholder/managers paid themselves less tax-deductible compensation relative to a control sample of nonmanagement-owned insurers (i.e., privately held insurers with no managerial ownership). The study's results expand our understanding of management-owned, privately held firms' tax-planning strategies, and have implications for the efficiency of the federal income tax system.

Keywords: *double taxation; income shifting; managerial compensation; privately held firms.*

Data Availability: *The data used in this study are from the public sources identified in the text.*

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I. INTRODUCTION

The objective of this study is to empirically investigate how taxes affect managerial compensation in privately held firms whose managers own a large percentage of the firm's stock (hereafter I refer to these firms as management-owned firms). I focus on management-owned firms because these firms face low manager/shareholder agency costs (Jensen and Meckling 1976; Ke et al. 2000). Thus, their managerial compensation is more likely to reflect tax considerations than are agency concerns.

Despite the theoretical argument for the importance of taxes in compensation arrangements of management-owned firms, empirical evidence has been limited because detailed firm-level data are seldom publicly available. Instead, the compensation literature has largely focused on publicly traded firms, where share ownership is highly dispersed and compensation data are accessible. This literature finds that taxes are not a significant determinant of managerial compensation in publicly traded firms. For example, Goolsbee (2000) documents that the significant increase in individual tax rates in 1993 encouraged corporate executives to accelerate the exercise of stock options from 1993 to 1992, but had little impact on the level of managerial compensation (see also Huddart 1998).[1] Hall and Liebman (2000) find that despite numerous changes in the tax advantages of stock options relative to cash compensation over the past two decades, the fraction of annual compensation from stock options is virtually unrelated to tax changes and is more closely related to changes in corporate governance (e.g., the increasing role of institutional investors). Rose and Wolfram (2000) further find that IRC §162(m), which capped publicly traded firms' tax deductibility of nonperformance-based compensation paid to the top five executives at $1 million, had little effect on the level and structure of executive compensation. These findings suggest that agency considerations dominate tax minimization in setting managerial compensation of publicly traded firms. However, there is no evidence on whether managerial compensation is more responsive to taxes when managerial agency costs are low.

The effect of taxes on compensation design also has public policy implications. Because shareholder/managers control significant amounts of economic resources, their responses to taxes affect Treasury revenue receipts and economic efficiency (Feldstein 1999; Slemrod 1995).[2] Recent presidential debates on tax policy have hinged on the way and magnitude in which high-income individuals respond to tax changes (see, e.g., Laffer and Moore 2000).

I use a sample of management-owned, privately held property-liability insurers (PL insurers) during 1989–1996 to test whether shareholder/managers adjust their own compensation to minimize the taxes on their total income from the firm. I focus on the insurance industry because firm-level data are available for privately held insurers. Similar information is not disclosed for privately held firms in most other industries.

Shareholder-managers receive two types of income from the firm they own: compensation income as employees, and investment income (i.e., dividends and capital gains) as shareholders. In the absence of tax influences, these two payments are independently determined. The labor market determines the level of compensation, whereas the capital market determines investment income.

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[1] Hall and Liebman (2000) further show that even the short-term timing of stock option exercises documented in Goolsbee (2000) appears to be due to correlated omitted variables rather than taxes.

[2] As indirect evidence of the importance of management-owned, privately held firms in the economy, the *Statistics of Income Bulletin* (1998) reports that more than 99 percent of the 4.47 million corporate federal income tax returns filed in 1995 had total assets under $25 million, well below the mean total assets of my sample of management-owned insurers. Ang et al. (2000) provide direct evidence that high managerial ownership in privately held firms is common in the U.S.

The U.S. federal income tax system taxes compensation income and investment income differently. Compensation income is fully taxable to individual employees, while it is deductible by employing firms. Investment income from a corporation is subject to so-called "double taxation." First, it is subject to corporate income tax; second, income after corporate taxes is further taxed at the shareholder level when individual shareholders receive dividends from the firm or sell their shares and realize capital gains. Thus, shareholder/managers have an incentive to reduce the double taxation on their investment income by paying themselves more tax-deductible compensation (e.g., salaries and bonuses).[3] This incentive is stronger when corporate tax rates increase or individual tax rates decrease.

Ordinary individual income tax rates were generally lower than corporate marginal tax rates during 1989–1992, but the 1993 Omnibus Budgetary Deficit Reconciliation Act significantly increased individual tax rates after 1992, while keeping corporate statutory tax rates essentially unchanged. Although the difference between individual and corporate tax rates suggests that shareholder/managers have an incentive to use tax-deductible compensation to mitigate double taxation throughout the entire 1989–1996 period, I predict that shareholder/managers paid themselves less deductible compensation in 1993–96 than in 1989–1992, *ceteris paribus.*

Compensation could change for both tax and nontax reasons. To isolate the effect of the 1993 tax rate change on the amount of deductible compensation shareholder/managers used to avoid the double taxation on their investment income, I use deductible managerial compensation of nonmanagement-owned insurers (i.e., insurers whose managers are not shareholders of the firm) as a benchmark. Because shareholders and managers are independent parties in nonmanagement-owned insurers, shareholders cannot use managerial compensation to reduce the double taxation on their investment income. Thus, nonmanagement-owned insurers represents a useful control group. However, the reader should interpret the study's empirical results with caution because this may not control for all determinants of managerial compensation correlated with ownership structure.

Consistent with my prediction, I find that relative to nonmanagement-owned insurers, the level of tax-deductible employee compensation that management-owned insurers paid declined significantly from 1989–1992 to 1993–96. The mean (median) reduction in deductible compensation over the two periods is $134,000 ($114,000) per year, representing 4.10 percent (2.42 percent) of the management-owned insurers' net premiums written. Furthermore, the reduction in shareholder/managers' compensation after 1992 was not limited to the two years adjacent to the tax law change, suggesting that their responses were persistent.

The rest of this paper is organized as follows. Section II briefly summarizes the changes in federal income taxation during 1989–1996. Section III discusses how privately held insurers are expected to respond to the 1993 Tax Act, and presents the research hypothesis. Section IV describes the sample selection procedure and provides descriptive statistics. Section V presents the research design and variable definitions. Section VI reports the main regression results and sensitivity checks, and Section VII concludes.

II. RECENT HISTORY OF U.S. FEDERAL INCOME TAXATION

Corporations and individual taxpayers face different statutory tax rates in the United States. Panel A of Table 1 shows a chronological history of the top individual and corporate

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[3] Increasing interest or rent payments to shareholders also mitigates double taxation. Because both debt financing and rental expenses are immaterial for property and liability insurers, this study does not consider them (see Cloyd et al. 1997; Ayers et al. 2001).

TABLE 1
Statutory Tax Rates for Individuals and Corporations

Panel A: Top Individual and Corporate Statutory Tax Rates: 1981–1996[a]

Year	Top Individual Statutory Rate	Top Corporate Statutory Rate	Difference
1981–86	50	46	4
1987	38.5	40	−1.5
1988–1990	28	34	−6
1991–92	31	34	−3
1993–96	39.6	35	4.6

Panel B: Tax Rate Schedules for Married Individuals Filing Joint Returns[b]

Taxable Income			
More than	But not over	1989	1994
0	29,750	15	15
29,750	36,900	28	15
36,900	89,150	28	28
89,150	140,000	28	31
140,000	250,000	28	36
250,000		28	39.6

Panel C: Tax Rate Schedules for Corporations[b]

Taxable Income			
More than	But not over	1989	1994
0	50,000	15	15
50,000	75,000	25	25
75,000	15,000,000	34	34
15,000,000		34	35

[a] Source: Gordon and Slemrod (2000, Table 8.1).
[b] Source: Internal Revenue Code 1986, as amended.

statutory federal tax rates since 1981. The top individual statutory tax rate was significantly higher than the top corporate statutory tax rate before 1987. For example, in 1985, the top individual statutory tax rate was 50 percent, while the top corporate statutory tax rate was 46 percent. The Tax Reform Act of 1986 lowered the top statutory tax rates for both individuals and corporate taxpayers and made the top statutory tax rate lower for individuals (28 percent) than for corporations (34 percent) for the first time in U.S. history. In 1991, the top individual tax rate increased to 31 percent, but it was still lower than the top corporate tax rate. Under the 1993 Budgetary Deficit Reconciliation Act, the top individual and corporate tax rates increased to 39.6 percent and 35 percent, respectively, thus restoring the pre-1987 ordering of the top individual and corporate tax rates.

Panel B provides the progressive tax rate schedules for married individuals filing joint returns for 1989 and 1994, representing the periods 1989–1992 and 1993–1996, respectively. Reflecting changes in the 1993 Tax Act, individual tax rates were uniformly higher in 1994 than in 1989 for taxable income above $89,150 (nominal dollars).[4] The difference in the individual tax rates over the two years ranges from 3 percent to 11.6 percent for taxable income above $89,150. Panel C shows the progressive tax rate schedules for taxable corporations for 1989 and 1994. Except for the one percentage point increase for taxable income over $15 million, the corporate tax rates were the same for the two years.[5]

The information in Panels B and C leads to three conclusions. First, with a taxable income threshold of only $75,000 for the 34 percent rate, most corporations faced a marginal tax rate of at least 34 percent during 1989–1996.[6] Second, individual marginal income tax rates should be lower than corporate marginal tax rates during 1989–1992. Third, because individual tax rates in 1993–1996 exceeded those in 1989–1992 for taxable income of more than $89,150, individual marginal tax rates should have increased after 1992 for managers in my sample of management-owned insurers (relatively high-income individuals).[7] To the extent that shareholder/managers' marginal tax rates did not increase after 1992, I would not observe a decrease in their deductible compensation after 1992 as predicted.

III. RESEARCH HYPOTHESIS

In this section I discuss various tax strategies that management-owned and nonmanagement-owned insurers could take in response to changes in individual and corporate tax rates over the 1989–1996 period. I also explain why nonmanagement-owned insurers can serve as a useful control group to identify the effect of tax changes on the amount of deductible compensation shareholder/managers used to avoid the double taxation on their investment income from the firm. Finally, I discuss the benefits and costs of using deductible compensation to shift income, and present the research hypothesis.

Strategies Common to Both Management-Owned and Nonmanagement-Owned Insurers

Assume that the amount of compensation that managers of privately held insurers can receive is limited to the true value of their personal services to the firm, which I describe as "reasonable compensation." Due to low managerial agency costs, *all* privately held firms could alter the form and timing of their managers' reasonable compensation to minimize taxes (Scholes and Wolfson 1992, Chapter 7). For example, because individual tax rates increased from 1989–1992 to 1993–96 while corporate tax rates remained almost unchanged, all privately held insurers could shift part of their managers' compensation from 1993 to 1992. In addition, all privately held insurers could increase the ratio of taxable compensation (e.g., salaries and bonuses) to tax-favored compensation (e.g., fringe benefits) during 1989–1992 and reduce this ratio during 1993–96 (see also Gordon and Slemrod 2000).

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[4] Because this study focuses on shareholder/managers who are likely to earn more than $36,900, I ignore the individual tax rate difference between 1989 and 1994 for taxable income of less than $36,900.

[5] During 1989–1996, the lower tax rates of 15 percent and 25 percent for corporations were phased out by imposing a surtax of 5 percent for taxable income between $100,000 and $335,000. During 1993–96, the 1993 Budgetary Deficit Reconciliation Act phased out the 34 percent rate by imposing a surtax of 3 percent for taxable income between $15,000,000 and $18,333,333. For simplicity, the following discussion ignores the surtax.

[6] Section IV provides direct evidence that taxable income before taxable employee compensation exceeds $75,000 for most PL insurers.

[7] To check the reasonableness of this assumption, I obtained from Ke et al. (2000) the CEO compensation figures in 1994 and 1995 for ten of my management-owned insurers. For these ten insurers, the mean (median) CEO compensation was $133,000 ($129,000) in 1994 dollars. This suggests that the CEO's *ex post* marginal tax rate was just below the 36 percent bracket in 1994. Based on my hypothesis, had the individual statutory tax rates not increased in 1993, the CEO's compensation should have been higher due to income shifting.

Strategy Unique to Management-Owned Insurers

In addition to reasonable compensation, shareholder/managers can receive additional income from the firm in the form of compensation (hereafter "unreasonable compensation"). This is because shareholder/managers receive both compensation and investment income from the same firm. Although the sum of the two received by shareholder/managers will likely be fixed due to competitive market forces, one could alter the mix of the two to minimize taxes. In contrast, managers of nonmanagement-owned insurers receive only reasonable compensation as employees. Therefore, their tax-deductible compensation can serve as a control to identify the amount of unreasonable compensation that shareholder/managers of management-owned insurers use to reduce the double taxation on their investment income.

Hypothesis

I now illustrate how a shareholder/manager's incentive for using unreasonable compensation to avoid double taxation on investment income changes with individual and corporate income tax rates. To highlight the difference in after-tax returns attributed solely to the different tax treatment of compensation and investment income, I assume the before-tax annual rate of return that the firm or individual shareholders can earn is a constant R. I denote the individual and corporate marginal tax rates τ_i and τ_c, respectively. Compensation income is taxed at the rate τ_i.

If the shareholder/manager used unreasonable deductible compensation to shift all before-tax rate of return R to her personal account, then she would avoid corporate tax and her annual rate of return after all taxes would be:

$$R(1 - \tau_i). \tag{1}$$

If the shareholder/manager retained the before-tax rate of return R within the firm, then the rate of return after corporate taxes would be $R(1 - \tau_c)$. However, the retained earnings would be subject to further shareholder taxes when the shareholder/manager received dividends directly from the firm or realized capital gains through the sale of her stock. Generally, shareholder taxes on dividends or capital gains depend on both the timing of the dividends or capital gains and the individual tax rate when the dividends or capital gains are received. I capture the actual amount of tax burden that the shareholder/manager must pay on the future dividends and capital gains using a hypothetical effective annualized tax rate (denoted τ_a) that she would pay each year on the returns after corporate taxes (see Scholes and Wolfson 1992, 73). Thus, if the firm retained corporate earnings, then the shareholder/manager's annual rate of return after both corporate and shareholder taxes would be:

$$R(1 - \tau_c)(1 - \tau_a). \tag{2}$$

Absent any costs of income shifting, the shareholder/manager would use deductible compensation to shift corporate income to her personal tax base if the following condition held:

$$(1 - \tau_i) > (1 - \tau_c)(1 - \tau_a). \tag{3}$$

Using reasonable assumptions for τ_c and τ_a in equation (3), I calculate the values of τ_i at which the shareholder/manager is indifferent between retaining corporate income within the firm and shifting it to her personal account for the sample period 1989–1996 (see Table 2). The corporate tax rate τ_c is assumed to be a constant 34 percent because it remained relatively unchanged during 1989–1996. I allow the annualized effective tax rate τ_a to vary from 0 percent to 28 percent. Generally, τ_a depends on both the shareholder/manager's investment horizon and future tax rates on dividends and capital gains. However, because dividends are taxed more heavily than long-term capital gains, I assume that management-owned insurers do not distribute corporate earnings in the form of dividends. As a result, the maximum rate for τ_a is 28 percent, the maximum long-term capital gains tax rate during 1989–1996. Allowing a higher value for τ_a increases the shareholder/manager's incentive to shift corporate income to her personal tax base but will not change any of the following discussions.[8]

The maximum individual marginal tax rate was always lower than 34 percent (the assumed corporate tax rate in Table 2) during 1989–1992. Thus, it is clear from Table 2 that the shareholder/manager had an incentive to shift corporate income to her personal tax base during 1989–1992, absent costs of income shifting. Further, the shareholder/manager might have found it optimal to shift corporate income to her personal tax base in 1993–96 because the maximum marginal tax rate was only 39.6 percent. However, the tax benefits of shifting income to shareholders was reduced from 1989–1992 to 1993–1996 because the individual tax rate increased significantly after 1992.

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[8] I assume that shareholders do not pay dividend tax and capital gains tax on the same income when they sell shares and realize capital gains (i.e., dividend tax and capital gains tax are substitutes). Collins and Kemsley (2000) suggest that shareholders who sell shares on a secondary market may pay both dividend tax (implicitly) and capital gains tax (directly) because dividend tax has already been capitalized in the stock price. Following their assumption will not affect my analyses.

TABLE 2

Determining the Individual Marginal Tax Rate (τ_i) at which Shareholder/Managers of Management-Owned Insurers are Indifferent between Shifting Corporate Income to Their Personal Tax Bases and Retaining Corporate Income within the Firm[a]

Assumptions for τ_c and τ_a	*The value for $\tau_i = 1 - (1 - \tau_c)(1 - \tau_a)$*
$\tau_c = 34\%$, $\tau_a = 0\%$	34%
$\tau_c = 34\%$, $\tau_a = 10\%$	40.6%
$\tau_c = 34\%$, $\tau_a = 20\%$	47.2%
$\tau_c = 34\%$, $\tau_a = 28\%$	52.5%

[a] τ_c is the corporate marginal tax rate; τ_a is a hypothetical effective annualized tax rate for the total individual taxes that a shareholder actually pays on her dividends and capital gains in the future; τ_i is the individual marginal tax rate.

Tax rule restrictions and transaction costs constrain shareholder-managers' ability to convert investment income to compensation income. For example, the IRS has the power to disallow the deduction of unreasonable compensation on the corporate tax return, but still requires the shareholder/manager to recognize it as ordinary income (see Wilkie et al. 1996). Thus the income-shifting strategy is inferior to the strategy of retaining investment income within the firm if the probability of an IRS challenge is high.

Furthermore, the income-shifting strategy becomes more complicated when there are multiple shareholder/managers with different marginal tax rates, or a mix of shareholder/managers and non-shareholder/managers. When there is only one shareholder who is also a manager of the firm, the shareholder/manager should have virtually full flexibility to use deductible compensation to change the mix of his compensation and investment income because both types of income accrue to the same person. However, if there are multiple independent shareholders, then the cost (benefit) of an increase (decrease) in compensation paid to a shareholder/manager would be borne by all shareholders. As a result, to make the income-shifting strategy work, shareholder/managers must find costly means to adjust the payoffs to other shareholders simultaneously so that they are not made worse off after the income-shifting strategy.[9] Thus, the tax rule restrictions and transaction costs may be sufficient to prevent management-owned insurers from garnering the tax benefits of income shifting.

The cost function of income shifting is unlikely to have changed while the tax benefits of income shifting decreased from 1989–1992 to 1993–96 due to the significant increase in individual tax rates. Therefore the shareholder/manager should have used deductible compensation to shift less corporate income to her personal tax base after 1992. Using deductible managerial compensation of nonmanagement-owned insurers to control for the common nontax changes affecting managerial compensation, the above analysis leads to the following prediction:[10]

> **H1:** Relative to nonmanagement-owned insurers, management-owned insurers paid less deductible compensation to their shareholder/managers during 1993–96 than during 1989–1992, *ceteris paribus.*

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[9] Although it is unknown exactly how shareholder/managers adjust the payoffs to other shareholders as a result of the income-shifting strategy using deductible compensation, Gordon and Slemrod (2000) discuss a few possibilities. One idea is to issue new shares to shareholders negatively affected by the income-shifting strategy. For example, consider the case in which a shareholder/manager retains a portion of her reasonable compensation within the firm due to high individual tax rates. Because the extra retained earnings accrue to all shareholders, not only to the shareholder/manager, the shareholder/manager could be compensated by receiving new shares. In principle, these shares should generate taxable income for the shareholder/manager based on their fair market value, and an equivalent tax deduction for the firm. In practice, however, the market value is difficult to ascertain because shares of privately held firms do not trade publicly, thus allowing considerable flexibility for undervaluation.

[10] Assuming that the total income (i.e., compensation and investment income) a shareholder/manager receives from the firm is fixed due to competitive market forces, an interesting implication of H1 is that the shareholder/manager's investment income should change inversely with their unreasonable compensation. I did not test this implication because shareholders' investment income (realized and unrealized) is more difficult to measure than compensation income in privately held firms. The only measure of shareholder return, accounting rate of return, is problematic because it does not include unrealized return and could be manipulated (e.g., for tax minimization) by both management-owned insurers and nonmanagement-owned insurers.

IV. SAMPLE SELECTION AND DESCRIPTIVE STATISTICS

Data and Sample Selection

I drew the firm-level data used in this study from the NAIC property and liability (PL) insurance database during 1989–1996.[11] The NAIC database contains PL insurers' annual statutory accounting statements filed with state insurance regulators.

PL insurers operate either independently or as a group whose members are owned by a common parent and usually share the same management team. Whereas the majority of stock insurance groups are publicly traded, this study drew its sample of privately held insurers from independent PL stock insurers (see Table 3). Of the initial sample of 408 independent stock insurers incorporated before 1990, I deleted 138 insurers due to lack of ownership information. I excluded an additional 69 insurers due to either ownership change or financial distress.[12] Mayers and Smith (1992) indicate that stock insurers owned by mutual insurers behave differently from other stock insurers. As a result, I excluded 8 additional mutually owned stock insurers. I deleted 21 additional insurers owned by publicly traded companies, and 32 others because the same firm does not have at least one observation for each of the two periods 1989–1992 and 1993–1996. The final sample includes 64 management-owned insurers and 76 nonmanagement-owned insurers.

Management-Owned vs. Nonmanagement-Owned

I classify an insurer as management-owned if top management collectively owns more than 25 percent of the stock. If top management owns no stock, then I classify the insurer as nonmanagement-owned. The ownership information was hand-collected from A. M. Best's *Insurance Report: Property and Casualty* (1989–1997 editions). As indicated by this

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[11] Data source: National Association of Insurance Commissioners (NAIC), used by permission. The NAIC does not endorse any analysis or conclusions based on the use of these data.

[12] Distressed insurers are those that are in receivership, conservationship, or are being liquidated.

TABLE 3
Sample Selection Criteria

U.S.-owned stock insurers in the 1992 NAIC property and liability insurer database with group code zero, nonmissing data during any of the years 1989–1996, and incorporation date before 1990	408
Less:	
With no ownership information from A.M. Best's *Insurance Reports: Property and Casualty* (1989–1997)	138
Ownership changes during 1989–1996	17
Distressed insurers[a]	52
Owned by a mutual insurer	8
Owned by a publicly traded corporation	21
No observations in both 1989–1992 and 1993–1996	32
Final sample	**140**
Management-owned privately held insurers	64
Nonmanagement-owned privately held insurers	76

[a] Distressed insurers are firms that were placed in receivership, in conservationship, or were being liquidated (as disclosed in Best's *Insurance Reports*) during the period 1989–1996.

report, there are no insurers in the sample whose managers own between 0 and 25 percent of the stock.

For the sample of 64 management-owned insurers, top management owns 100 percent of the stock in 47 insurers, between 50 percent and 99 percent in 12 insurers, and between 25 percent and 49 percent in 5 insurers. None of the following regression inferences change if the management-owned insurer sample includes only those firms whose top management owns at least 50 percent or 100 percent of the stock.

Descriptive Statistics

Table 4 reports descriptive statistics by ownership structure. Except for TAXINC (see below for an explanation), I express all variables in 1989 constant dollars. The variables in Table 4 are means over 1989–1996 for each insurer. There are 444 (551) firm-years for the sample of management-owned insurers (nonmanagement-owned insurers). Of the 140 unique firms, 122 (106) firms have at least two (three) years of observations in each of the two periods 1989–1992 and 1993–96.[13]

Management-owned and nonmanagement-owned insurers differ on several dimensions. Management-owned insurers are significantly smaller, and younger, and operate in fewer states than do nonmanagement-owned insurers.[14] However, return on assets before deductible compensation (ROA) is similar for the two groups. Workers' average annual pay (COST_OF_LIVING) in the insurers' states of domicile is similar for the management-owned and nonmanagement-owned insurers.

COMPENSATION is total tax-deductible compensation paid to all company employees (i.e., salaries and bonuses) obtained from schedule "Part 4—expenses" (line 8a) in PL insurers' annual statements. Because PL insurers do not disclose managerial compensation separately in their annual statements, the later regression analysis uses COMPENSATION as the dependent variable (see Section V for further discussion). Because management-owned insurers are smaller, COMPENSATION is smaller than that of nonmanagement-owned insurers ($p < .001$).

Table 4 also provides an estimate of total taxable income before COMPENSATION, averaged over 1989–1996 for management-owned and nonmanagement-owned insurers. Taxable income is the sum of taxable investment income and underwriting gain. Because I do not index the corporate tax rate schedule for inflation in Table 1, I express corporate taxable income in nominal dollars in Table 4. Untabulated calculation indicates that taxable income before COMPENSATION over 1989–1996 is more than $75,000 (the threshold for the 34 percent tax rate) for 89 percent of the management-owned insurers and 88 percent of nonmanagement-owned insurers. This result suggests that most of the sample firms faced high corporate tax rates during 1989–1996.

V. RESEARCH DESIGN

Regression Model

To test whether shareholder/managers of management-owned insurers use deductible compensation to shift corporate income to their personal tax bases to avoid double taxation

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[13] The following regression results were unaltered if firms with only two years of observations over 1989–1996 were deleted.

[14] ASSET (total admitted assets) in Table 4 is total assets prepared under the statutory accounting principles (SAP) that all domestic insurers follow. A major difference between SAP and generally accepted accounting principles (GAAP) is that certain illiquid or risky assets (e.g., real estate investments not meeting insurance regulation) recognized under GAAP are not recognized under SAP to reflect the conservatism of insurance accounting. See Galloway and Galloway (1986, 23) for a detailed discussion.

TABLE 4
Descriptive Statistics by Ownership Structure (means, medians, and standard deviations)

Variables	*Management-Owned Insurers (n = 64)*	*Nonmanagement-Owned Insurers (n = 76)*	*Rank Sum Test z-value (p-value)*
ASSET (millions)	29.53	106.36	−5.39
	(8.50)	(36.26)	(0.001)
	[100.34]	[223.01]	
PREMIUMS (millions)	11.81	28.84	−4.84
	(4.52)	(12.89)	(0.001)
	[39.89]	[43.75]	
AGE	14.57	23.40	−4.06
	(8.13)	(15.75)	(0.001)
	[16.87]	[20.66]	
LICENSE	6.17	13.40	−1.95
	(2.27)	(3.80)	(0.05)
	[9.68]	[16.93]	
ROA	0.10	0.09	1.27
	(0.10)	(0.08)	(0.10)
	[0.05]	[0.06]	
COST_OF_LIVING (thousands)	26.20	26.33	−0.37
	(25.66)	(25.46)	(0.71)
	[3.50]	[3.20]	
COMPENSATION (millions)	1.18	2.59	−3.21
	(0.45)	(1.01)	(0.001)
	[3.23]	[5.28]	
TAXINC (millions)	2.03	5.35	−3.79
	(0.88)	(2.09)	(0.001)
	[4.66]	[10.73]	

The variables reported in the table are means over 1989–1996 for each insurer.

ASSET is total admitted assets at year-end. PREMIUMS is net insurance premiums written. COMPENSATION is the total employee compensation, including salaries, bonuses, and other immaterial emoluments. ROA is return on assets, measured by total earnings before taxes and tax-deductible compensation as a percentage of the average of beginning and ending total admitted assets. AGE is the number of years since incorporation. LICENSE is the number of states in which an insurer is licensed to do business. COST_OF_LIVING is workers' average annual pay in a state. TAXINC is estimated taxable income before employee compensation.

Except for TAXINC, all values are expressed in constant 1989 dollars.

on investment income (i.e., dividends and capital gains), I estimate the following regression model:

$$\begin{aligned}\text{COMPENSATION}_{it} = {} & a + b_1\text{MGR\_OWNED}_i + b_2\text{YR93\_96} \\ & + b_3\text{MGR\_OWNED}_i \times \text{YR93\_96} \\ & + b_4\text{PREMIUMS}_{it} + b_5\text{LICENSE}_{it} + b_6\text{ROA}_{it} \\ & + b_7\text{COST\_OF\_LIVING}_{it} + e_{it},\end{aligned} \tag{1}$$

where:

i = firm index;
t = year index;
COMPENSATION = total tax-deductible employee compensation;
MGR_OWNED = 1 for management-owned insurers, and 0 otherwise;
YR93_96 = 1 for the 1993–96 period, and 0 otherwise;
PREMIUMS = net premiums written (in millions);
LICENSE = the number of states in which an insurer is licensed to underwrite insurance;
ROA = return on assets, measured by earnings before taxes and total tax-deductible employee compensation, scaled by the average of beginning and ending total admitted assets;
COST_OF_LIVING = workers' average annual pay in an insurer's state of domicile; and
e = error.

Except for YR93_96, MGR_OWNED, and YR93_96 × MGR_OWNED, I express all variables in natural logarithms to control for the skewness of several variables and to facilitate the interpretation of the regression coefficients as elasticities.[15] The inferences are similar for nontransformed data. Because the descriptive statistics in Table 4 suggest that most privately held insurers had high corporate marginal tax rates during 1989–1996, regression model (1) does not model the variation in the true corporate marginal tax rate.

Dependent Variable

The dependent variable, COMPENSATION, is total tax-deductible compensation paid to all company employees (i.e., salaries and bonuses). Although the research hypothesis in Section III focuses on deductible compensation paid to shareholder/managers and non-shareholder/managers, due to data limitations I test the research hypothesis using total employee compensation. Including lower-level employee compensation in the dependent variable reduces the power of my hypothesis tests, but should not induce any bias. First, the marginal tax rates for lower-level employees were unlikely to change much during 1989–1996. Second, lower-level employees' utility functions are likely too diverse to allow the firm to design an efficient and uniform tax-minimizing strategy. Third, because both management-owned insurers and nonmanagement-owned insurers employ lower-level employees, any feasible tax strategy for lower-level employees should be common to both groups. In addition, lower-level employees are unlikely to be shareholders of the firm, limiting the amount of taxes avoided using their compensation.

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[15] Because ROA is negative or 0 for some firm years, I added 1 to ROA before taking the logarithm.

Control Variables

I include several variables to control for the exogenous nontax determinants of COMPENSATION. PREMIUMS controls for firm size and LICENSE controls for the number of states in which an insurer is licensed to conduct business.[16] Because firms that are larger and operate in more states are expected to incur more compensation expense (Rosen 1992), the coefficients on PREMIUMS and LICENSE are predicted to be positive. COST_OF_LIVING controls for the cost of living in an insurer's state of domicile and is predicted to be positive.[17] ROA controls for performance-related compensation. Although Ke et al. (2000) find that CEO compensation is not sensitive to firm performance for privately held insurers, I include ROA as a control because my dependent variable COMPENSATION includes deductible compensation paid to all employees. The coefficient on ROA should be positive if there is a pay-for-performance relation in my sample firms.[18] I do not allow the coefficient on ROA to vary with MGR_OWNED because share ownership is highly concentrated in both types of insurers, and thus the pay-for-performance sensitivity (if there is any) should not vary across the two groups. Regression results are qualitatively the same if I allow the coefficient on ROA to vary with MGR_OWNED. Further, the coefficient on MGR_OWNED × ROA is never significant.

Predictions Related to H1

If all exogenous nontax determinants of COMPENSATION are controlled, then the coefficient on MGR_OWNED captures the tax-induced difference in COMPENSATION between management-owned insurers and nonmanagement-owned insurers for the period 1989–1992. Similarly, the sum of the coefficients on MGR_OWNED and MGR_OWNED × YR93_96 captures the tax-induced difference in COMPENSATION between management-owned insurers and nonmanagement-owned insurers for the period 1993–1996. The coefficient on MGR_OWNED × YR93_96 measures the incremental tax-induced change in COMPENSATION from 1989–1992 to 1993–1996 for management-owned insurers relative to nonmanagement-owned insurers. Hypothesis 1 predicts the coefficient on MGR_OWNED × YR93_96 to be negative. Assuming all nontax determinants of COMPENSATION are controlled, a positive coefficient on MGR_OWNED (a positive sum of the coefficients on MGR_OWNED and MGR_OWNED × YR93_96) would be consistent with shareholder/managers of management-owned insurers using unreasonable deductible compensation to shift corporate income to their personal tax bases during 1989–1992 (1993–1996).

Realistically, the regression model is unlikely to control for all nontax determinants of COMPENSATION, but to the extent that these omitted variables are uncorrelated with MGR_OWNED, the regression coefficients should still be consistent. One potential correlated omitted variable is number of employees. Although PREMIUMS and total assets are highly correlated with number of employees, they may not be adequate proxies for the number of employees. Similarly, the ratio of managers to lower-level employees could systematically differ across management-owned insurers and nonmanagement-owned insurers.

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[16] Two alternative size measures are number of employees and total admitted assets. Data on the number of employees are unavailable (but see below for an explanation of how the fixed-effects regression controls for the number of employees). The regression results using only total admitted assets are qualitatively the same, but because net premiums written explains substantially more variation in total compensation than total admitted assets, I use net premiums written in the reported regressions. Further, the regression results were qualitatively the same if I included both net premiums written and total admitted assets. Because the two size measures are highly correlated, I report the main results using net premiums written, alone.

[17] I hand-collected the data on COST_OF_LIVING from the *Statistical Abstract of the United States* (1989–1998 editions). I thank Andy Leone for suggesting the variable.

[18] Note that a positive relation between pay and performance does not invalidate my argument that managerial compensation is structured to reduce taxes in management-owned insurers. All I need for my research hypothesis is that agency conflicts do not dominate tax minimization in the design of managerial compensation in management-owned insurers.

Unfortunately, data on the number of employees or on the ratio of managers to lower-level employees are unavailable. However, both the number of employees and the ratio of managers to lower-level employees (or any other firm fixed effects) are likely to be stable over time; thus I use fixed-effects regression to control for these correlated omitted variables. Despite numerous controls for the nontax determinants of employee compensation and alternative specifications of the dependent variable in later sensitivity checks, one should interpret the following empirical results with caution. As indicated above, I omitted some important determinants of compensation from the regression model and simply treated them as firm fixed effects. To the extent that these omitted variables change over time and my existing time-varying control variables do not adequately control for these omitted variables, the estimated regression coefficients could be biased.

VI. EMPIRICAL RESULTS

Pooled Regression Results

Table 5 reports the pooled regression results for the full sample using both OLS regression and fixed-effects regression methods.[19] I cannot estimate the coefficient on MGR. OWNED in the fixed-effect regression because it is time-invariant.

Column A of Table 5 reports the pooled OLS regression result for the full sample. YR93_96 is significantly positive, suggesting that there was an upward nontax trend in employee compensation from 1989–1992 to 1993–96. This finding highlights the importance of using a control sample to separate the effect of tax changes from that of nontax changes.

The positive coefficient on MGR_OWNED is consistent with the prediction that shareholder/managers used deductible compensation to avoid the double taxation on investment income during 1989–92. Consistent with H1, the coefficient for MGR_OWNED × YR93_96 is significantly negative ($p = 0.03$, one-tailed), suggesting that shareholder/managers of management-owned insurers used deductible compensation to shift less corporate income to their personal accounts when individual tax rates increased from 1989–1992 to 1993–96. The sum of the coefficients on MGR_OWNED and MGR_OWNED × YR93_96 is only 0.043 and not statistically significant ($p = 0.65$, two-tailed); thus there is no clear evidence that shareholder/managers used deductible compensation to shift corporate income to their personal tax bases during 1993–96.

The coefficients on the control variables from the OLS regression are all significant and consistent with expectations. Firms that are larger and that operate in more states incur more employee compensation expense. Insurers domiciled in states with higher costs of living incur more compensation expense. Total employee compensation is also significantly associated with firm performance.

The fixed-effects regression coefficients appear in Column B of Table 5. Consistent with the OLS regression result, the coefficient on MGR_OWNED × YR93_96 remains significantly negative ($p = 0.001$, one-tailed), although the absolute magnitude is reduced slightly. The coefficients on the control variables also remain significant except for the

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[19] Influential observations are deleted using Cook's (1977) distance criteria for all regressions in the paper.

TABLE 5
Regressions of Log of Total Deductible Compensation (p-values in parentheses)

	Prediction	*A*	*B*
YR93_96	?	0.221 (0.012)*	0.187 (0.000)**
MGR_OWNED	+	0.241 (0.017)*	
MGR_OWNED × YR93_96	−	−0.197 (0.029)*	−0.166 (0.001)**
PREMIUMS	+	0.768 (0.000)**	0.375 (0.000)**
LICENSE	+	0.061 (0.035)*	0.004 (0.461)
ROA	+	1.162 (0.019)*	0.596 (0.011)*
COST_OF_LIVING	+	0.748 (0.013)*	1.437 (0.000)**
Constant	?	−4.647 (0.000)**	−5.840 (0.000)**
n		927	927

*, ** Denote significance levels of 0.05 and 0.01, respectively. The significance tests are one-tailed if there is a prediction and two-tailed otherwise.

Column A reports the OLS regression coefficients, while column B reports the firm fixed-effects regression coefficients.

YR93_96 is 1 for the 1993–96 period, and 0 otherwise. MGR_OWNED is 1 for management-owned insurers, and 0 otherwise. LICENSE is number of states in which an insurer is licensed to write insurance. PREMIUMS is net premiums written. COST_OF_LIVING is workers' average annual pay in a state.

All the other variables are defined in Table 4.

Except for the dummy variables, all the variables are expressed in natural logarithm.

Influential observations are deleted using Cook's (1977) distance criteria.

The p-values are corrected for heteroskedasticity and time serial correlation using STATA's cluster command (see Rogers 1993).

coefficient on LICENSE. The insignificant coefficient on LICENSE is not surprising because LICENSE does not vary much over time and thus loses significance in a fixed-effects regression.[20]

To assess the economic magnitude of the coefficient on MGR_OWNED × YR93_96, I also calculate the dollar value of the changes in deductible compensation that shareholder/managers of management-owned insurers made in response to the 1993 individual tax rate increase. Using the fixed-effects regression coefficients from Column B of Table 5, untabulated calculation indicates that shareholder/managers (as a whole) of management-owned insurers reduced their deductible compensation (in 1989 dollars) by a mean (median) of \$134,000 (\$114,000) per year after 1992. The mean (median) reduction in deductible compensation represents about 4.10 percent (2.42 percent) of the annual insurance premiums written.

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[20] As a robustness check, I also allowed the coefficients on all firm-specific control variables (i.e., PREMIUMS, LICENSE, and ROA) to vary with MGR_OWNED. This specification essentially allows differential nontax trends related to the control variables between the two types of insurers. The fixed-effects regression coefficient on MGR_OWNED * YR93_96 remained highly significant (coefficient = −0.15, p = 0.002, one-tailed).

Persistence of Management-Owned Insurers' Responses

The pooled regression results reported so far do not indicate whether management-owned insurers' reduction in tax-deductible managerial compensation after 1992 was a temporary or a persistent response. Such distinction is important for understanding the effect of the tax change on future tax revenues. In addition, the distinction has implications for tax incidence (i.e., who bears the tax burden) and efficiency, the two most important issues in the economic analysis of taxation (see Slemrod 1995).

To address this question, I reran the fixed-effects regression after excluding years 1992 and 1993, the two years immediately before and after the 1993 tax change (see Column A of Table 6). As a comparison, I also report the fixed regression result in Column B of Table 6 for the years 1992 and 1993. The coefficient on MGR_OWNED × YR93_96 is significantly negative ($p = 0.002$, one-tailed) for the regression in Column A, suggesting that management-owned insurers' adjustment to managerial compensation represented a more persistent response to the 1993 tax rate increase.

As a robustness check, I also estimate the regression model by year using OLS (results not tabulated). This allows the regression coefficients on all independent variables to vary over time, although the coefficient on MGR_OWNED may also capture nontax effects. Consistent with the results in Table 6, the coefficient for MGR_OWNED is significantly positive in three of the first four years ($p < 0.10$, one-tailed), but insignificantly different from 0 in three of the last four years. More importantly, the coefficients on MGR_OWNED generally are larger in the first four years than in the last four years, consistent with the pooled regressions in Table 6.

Alternative Specifications

The regression results reported so far use COMPENSATION as the dependent variable. To check the robustness of the regression results, I used two alternative specifications to estimate the regression model. First, I estimated regression model (1) in the change specification (Panel A, Table 7). As I expect management-owned insurers' income-shifting incentive to decline from 1989–1992 to 1993–96 but not to change within each of the two periods, I examine changes in COMPENSATION only from 1989–1992 to 1993–96. The change of each variable in regression model (1) is defined as the difference in means from 1989–1992 to 1993–96.

In contrast to the COMPENSATION levels regression, the change specification directly estimates the changes in COMPENSATION over the two periods for each firm. As a result, the change model provides further assurance on the robustness of the levels regression. A limitation of the change model is that it is less powerful because the variables are averaged over several years. Consistent with the pooled regression results reported in Table 5, the coefficient on MGR_OWNED, which captures the changes in COMPENSATION for management-owned insurers relative to nonmanagement-owned insurers, is significantly negative ($p < 0.03$, one-tailed).

TABLE 6
The Effect of the 1993 Tax Rate Change on Total Deductible Compensation: Persistence of the Responses
(p-value in parentheses)

	Prediction	*A*	*B*
YR93_96	?	0.234 (0.004)**	0.124 (0.054)
MGR_OWNED × YR93_96	−	−0.172 (0.002)**	−0.116 (0.063)
PREMIUMS	+	0.375 (0.000)**	0.328 (0.001)**
LICENSE	+	0.046 (0.180)	−0.015 (0.453)
ROA	+	0.687 (0.015)*	0.515 (0.261)
COST_OF_LIVING	+	1.109 (0.005)**	1.916 (0.141)
Constant	?	−4.854 (0.000)**	−7.288 (0.209)
n		678	249

---

*, ** Denote significance levels of 0.05 and 0.01, respectively. The significance tests are one-tailed if there is a prediction and two-tailed otherwise.

Column A (B) includes only firm years other than 1992 and 1993 (firm years 1992 and 1993).

See Tables 4 and 5 for variable definitions.

Regression coefficients from fixed-effects regressions.

Except for the dummy variables, all the variables are expressed in natural logarithm. Influential observations are deleted using Cook's (1977) distance criteria.

The p-values are corrected for heteroskedasticity and time serial correlation using STATA's cluster command (see Rogers 1993).

Second, I used the ratio of total deductible employee compensation to net premiums written (COMPENSATION_PREMIUMS) as the dependent variable (Panel B, Table 7). Because COMPENSATION_PREMIUMS is likely to be a function of firm size (Joskow 1973), the model controls for PREMIUMS. The inference from the fixed-effects regression for COMPENSATION_PREMIUMS is similar to those in Table 5. The coefficient on MGR_OWNED × YR93_96 is still significantly negative ($p = 0.03$, one-tailed).

VII. CONCLUSION

This study investigates the influence of taxes on managerial compensation in privately held firms whose managers own a high percentage of the firm's stock (I describe these as management-owned firms). Shareholders suffer double taxation on their investment income from taxable corporations. The firm first pays corporate taxes on corporate income; they pay additional individual taxes on the dividends or capital gains when shareholders receive dividends or sell shares. Shareholder/managers of management-owned firms can avoid the

TABLE 7
Sensitivity Analyses: Alternative Definitions of the Dependent Variable

Panel A: The Dependent Variable is Changes in the Average of the Log of Employee Compensation from 1989–1992 to 1993–1996[a]

	Prediction	*OLS Regression Coefficient (p-value)*
MGR_OWNED	−	−0.202 (0.024)*
ΔPREMIUMS	+	0.499 (0.000)**
ΔLICENSE	+	−0.029 (0.400)
ΔCOST_OF_LIVING	+	−1.634 (0.268)
ΔROA	+	−0.409 (0.318)
Constant	?	0.581 (0.047)*
n		140

Panel B: The Dependent Variable is Total Employee Compensation over Net Premium Written[b]

	Prediction	*Fixed-Effects Regression Coefficient (p-value)*
YR93_96	?	0.014 (0.005)**
MGR_OWNED × YR93_96	−	−0.010 (0.029)*
PREMIUMS	−	−0.074 (0.000)**
LICENSE	+	−0.001 (0.448)
ROA	+	0.070 (0.006)**
COST_OF_LIVING	+	0.134 (0.000)**
Constant	?	−0.181 (0.049)*
n		947

ΔPREMIUMS is defined as changes in the average of the log of PREMIUMS from 1989–1992 to 1993–1996. ΔLICENSE, ΔCOST_OF_LIVING, and ΔROA are defined in the same way but for LICENSE, ΔCOST_OF_LIVING, AND ΔROA. See Table 5 for the definitions of other variables. All values are in 1989 dollars.

* and ** denote significance levels of 0.05 and 0.01, respectively. The significance tests are one-tailed if there is a prediction and two-tailed otherwise.

[a] OLS regression coefficients. There are no influential observations using Cook's (1977) distance criteria.

[b] Fixed-effects regression coefficients. Influential observations are deleted using Cook's (1977) distance criteria.

double taxation on their investment income (i.e., dividends and capital gains) by using deductible compensation to shift corporate income to their personal tax bases.

Using a sample of 64 management-owned, privately held insurers and 76 nonmanagement-owned, privately held insurers during 1989–1996, this study tests whether management-owned insurers structure their shareholder/managers' compensation to avoid double taxation. Because managers and shareholders are independent parties in nonmanagement-owned insurers, managers of nonmanagement-owned insurers can receive only reasonable compensation from the firm. Thus managerial compensation of nonmanagement-owned insurers provides a benchmark to identify the amount of deductible compensation that shareholder/managers of management-owned insurers use to avoid the double taxation on their investment income. Because individual marginal tax rates increased significantly from 1989–1992 to 1993–96 while corporate marginal tax rates remained essentially unchanged, I expect that shareholder/managers of management-owned insurers would use their compensation to shift less corporate income to their personal tax bases after 1992.

Because managerial compensation data are not available, I test the above prediction using total tax-deductible employee compensation. Consistent with my prediction, I find that management-owned insurers reduced the amount of deductible compensation by a mean (median) of $134,000 ($114,000) per year after 1992, representing 4.10 percent (2.42 percent) of the annual insurance premiums written. Furthermore, management-owned insurers' response to the 1993 tax change appears relatively persistent.

The results from this study are important for several reasons. First, management-owned firms are the base case in the agency theory of the firm (Jensen and Meckling 1976). In addition, Scholes and Wolfson (1992) use management-owned firms as a benchmark to illustrate how optimal tax planning deviates from tax minimization when there are incentive problems. In spite of the importance of management-owned firms in theory, the way these firms respond to taxes is little understood. This study addresses this gap in our knowledge of tax planning. The significant response of managerial compensation to tax changes in management-owned insurers suggests that incentive alignment does not dominate tax minimization in the design of managerial compensation in management-owned, privately held firms. The results also suggest that tax rule restrictions and transaction costs are not sufficient to prevent management-owned firms from structuring managerial compensation to minimize taxes.

Second, the estimated magnitude and persistence of management-owned insurers' responses to taxes also provide insight into likely revenue and efficiency effects of future tax changes. Because privately held firms are more responsive to taxes than publicly traded firms, tax policymakers may wish to consider the responses of privately held firms and publicly traded firms separately when analyzing possible effects of future tax policy changes.

An important limitation of this study is that it examined only one exogenous tax change in 1993; thus it is possible that the documented tax effect could be a result of differential nontax trends in compensation for management-owned and nonmanagement-owned insurers. As a result, an examination of the same issue using other time periods (e.g., the 1986 tax changes) seems warranted.

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Hyperlinking Unaudited Information to Audited Financial Statements: Effects on Investor Judgments

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ABSTRACT: This study provides evidence that hyperlinking a firm's audited financial statements to unaudited information in a web-based environment leads investors to blend the unaudited information with the audited statements. I obtain evidence of this blending effect using an experiment where investors assessed a firm's earnings potential by evaluating the firm's audited financial statements and a subsequent optimistic unaudited letter to shareholders from the firm's management. Investors who viewed hyperlinked materials on the Web misclassified more unaudited information as audited and assessed the credibility of the unaudited information as higher than did investors who viewed hardcopy materials. Those investors who assessed the unaudited information as more credible also judged the firm's earnings potential to be higher. Notifying users with an "AUDITED/NOT AUDITED" label attenuated these effects. This evidence suggests that firms can influence financial report users' perceptions by hyperlinking unaudited information to information in their audited financial statements, and that a simple disclosure rule reduces this influence.

Keywords: *World Wide Web; financial statement analysis; audited/unaudited; presentation format.*

Data Availability: *The data are available upon request.*

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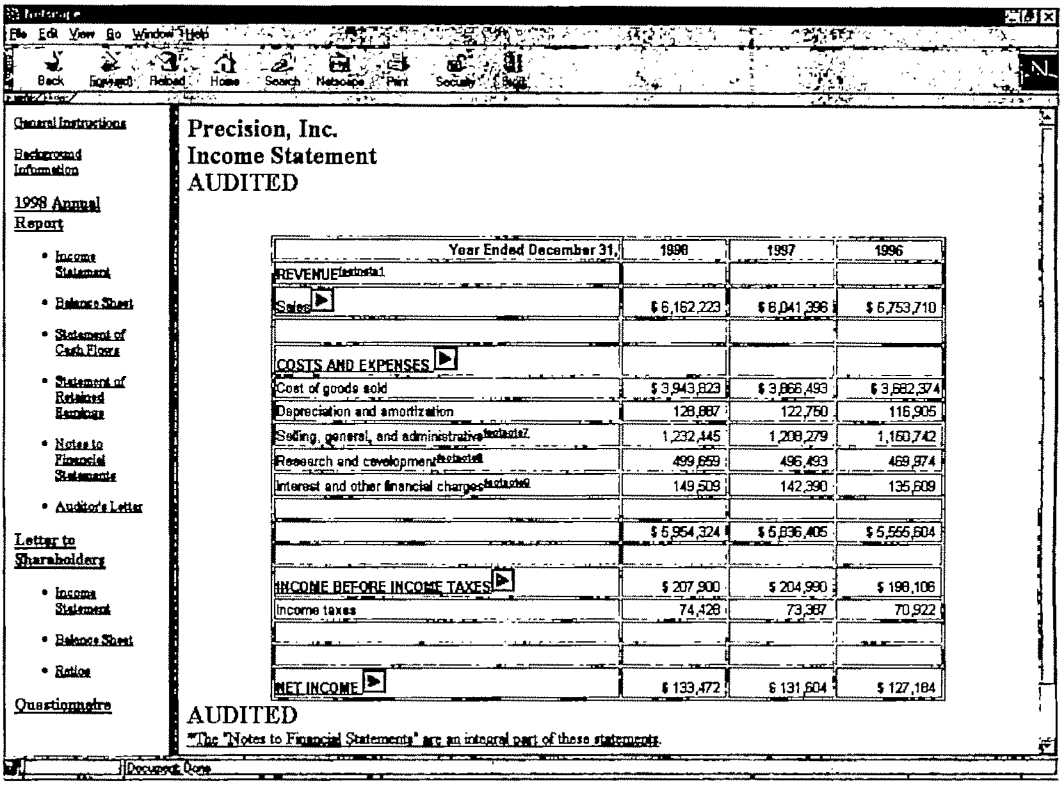
EXHIBIT 1
Example of WEB—with Aid Condition[a]



Precision, Inc.
Income Statement
AUDITED

Year Ended December 31,	1998	1997	1996
REVENUE[footnote1]			
Sales ▶	$ 6,162,223	$ 6,041,396	$ 6,753,710
COSTS AND EXPENSES ▶			
Cost of goods sold	$ 3,943,823	$ 3,866,493	$ 3,682,374
Depreciation and amortization	128,887	122,760	116,905
Selling, general, and administrative[footnote7]	1,232,445	1,208,279	1,160,742
Research and development[footnote8]	499,659	496,493	469,974
Interest and other financial charges[footnote9]	149,509	142,390	135,609
	$ 5,954,324	$ 5,836,405	$ 5,555,604
INCOME BEFORE INCOME TAXES ▶	$ 207,900	$ 204,990	$ 198,106
Income taxes	74,428	73,387	70,922
NET INCOME ▶	$ 133,472	$ 131,604	$ 127,184

AUDITED
"The "Notes to Financial Statements" are an integral part of these statements.

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[a] The WEB—with Links condition is identical, except that it does not include the two "AUDITED" labels.

the next three years is __________," using an 11-point scale with endpoints labeled "very low/very high."

Procedure

I conducted the experiment in two settings. Forty-one percent of the participants completed the experiment in a controlled classroom setting ("in-lab" participants). In the controlled setting, participants in the web-based conditions completed the materials in a computer lab, and participants in the hardcopy condition completed the materials in a regular classroom. The remaining participants (59 percent) completed the experiment at their convenience ("out-of-lab" participants). Out-of-lab participants in the web-based conditions and the hardcopy condition received the same experimental materials as their in-lab counterparts. The ratio of in-lab to out-of-lab participants for each condition was WEB—with links, 9/9, WEB—with aid, 7/6, HARDCOPY—no links, 5/11.

To examine whether in-lab and out-of-lab participants completed the materials in a similar manner (i.e., in one sitting and without looking back through the materials when answering questions) I compared the average amount of time each group spent completing the materials, and each group's average classification error rate. If out-of-lab participants did not complete the materials in one sitting, then the average amount of time they spent completing the materials should be greater than that of the in-lab participants. If out-of-lab participants looked back through the materials while completing the classification task, then their classification error rates should be lower than the error rates of in-lab participants. Results reflect no differences across these two measures. In-lab (out-of-lab) participants took 41 (38) minutes on average to complete the case ($t_{45} = 0.89$, $p = 0.376$, two-tailed), and in-lab (out-of-lab) participants on average misclassified 22 percent (24 percent) of the information ($t_{44} = 0.42$, $p = 0.680$, two-tailed). Thus, I conclude that both in-lab and out-of-lab participants followed the directions in completing the materials.

Participants in the hardcopy condition received three envelopes: one containing instructions, an introductory letter, and the case; one containing the main questionnaire; and one containing the post-experiment questionnaire. After evaluating the materials, participants in the hardcopy condition placed them back in Envelope 1. They then opened Envelope 2 and completed the main questionnaire by providing their earnings potential judgments and completing the information classification task. After completing this questionnaire and returning it to Envelope 2, they opened Envelope 3 and completed the post-experiment questionnaire. The post-experiment questionnaire asked participants how useful and credible they found the information, and how objective Precision's management was in creating the audited and unaudited reports. It also contained several manipulation checks and concluded with a demographic survey.

Participants in the two web-based conditions received a single piece of paper listing a web address for the case. After typing in the address they viewed the same instructions, letter of introduction, and case that hardcopy participants received. After evaluating the materials, participants in the two web-based conditions clicked on a "Ready to Begin Questionnaire" button that presented them with the main questionnaire. After completing this questionnaire, participants sent their responses to me via email by clicking on a button labeled "Submit Questionnaire." Participants in the web-based conditions then clicked on a "Questionnaire #2" button that presented them with the post-experiment questionnaire, which they completed and submitted in the same manner.

IV. RESULTS

Manipulation Checks

Data collected in the post-experiment questionnaire reveal that all participants recognized that Precision's financial statements were audited and that the letter was not. Participants also reported that they weighted the information in the financial statements more heavily relative to the information in the letter when making their judgments ($t_{46} = 6.59$, $p < 0.001$). ANOVA results indicate that these self-reported weights do not differ among the three experimental conditions ($F_{1,44} = 0.19$, $p = 0.831$). In addition, participants across conditions judged the unaudited information in the letter/*pro forma* statements as more optimistic than the audited information in the financial statements ($t_{45} = 10.10$, $p < 0.001$). Finally, the data reflect no significant differences among conditions in the number of accounting courses taken ($F_{1,43} = 0.57$, $p = 0.571$), the number of finance courses taken ($F_{1,43} = 1.43$, $p = 0.251$), or the number of times that participants had evaluated a set of financial statements ($F_{1,44} = 1.11$, $p = 0.339$).

Hypotheses Tests

Panel A of Table 1 reports the descriptive statistics for the three dependent measures.[10]

Classification Error Rates

Participants classified whether each of nine pieces of information originated from the financial statements or the letter to shareholders. A programming error rendered data from two of the nine classification questions useless. The hypotheses tests reported in this section rely on data from the remaining seven questions.[11]

Hypothesis 1a predicts that participants in the WEB—with links condition will misclassify more unaudited information as audited than the reverse. Panel B of Table 1 reports that participants in the WEB—with links condition mistakenly classified 37 percent of the unaudited items as audited and only 22 percent of the audited items as unaudited ($t_{17} = 1.90$, $p = 0.038$, one-tailed). These results support H1a, and suggest that hyperlinking the unaudited and audited information led investors to blend the information in the unaudited letter into the audited financial statements, rather than the reverse. Additional analysis reveals no significant difference in the direction of classification errors committed by participants in the HARDCOPY—no links condition—they mistakenly classified 22 percent of the unaudited items as audited, and 15 percent of the audited items as unaudited ($t_{14} = 0.93$, $p = 0.367$, two-tailed).

Hypothesis 1b predicts that hyperlinking the audited and unaudited information will lead participants in the WEB—with links condition to misclassify more unaudited information as audited than participants in the HARDCOPY—no links condition. Panel C of Table 1 reports that participants in the WEB—with links condition misclassified more unaudited information as audited than did participants in the HARDCOPY—no links condition (37 percent vs. 22 percent, $t_{31} = 1.62$, $p = 0.058$, one-tailed). These results support H1b, and suggest that hyperlinking the unaudited and audited information made it more difficult for participants to classify the unaudited information correctly.

Information Credibility

Hypothesis 2a predicts that the more unaudited information participants misclassify as audited, the higher they will assess the credibility of the entire information set. Results reported in Panel D of Table 1 support H2a ($r_{45} = 0.25$, $p = 0.047$, one-tailed). Panel E of Table 1 reports that participants in the WEB—with links condition assessed the entire information set as more credible than did participants in the HARDCOPY—no links condition (6.47 vs. 5.13, $t_{31} = 2.23$, $p = 0.017$, one-tailed), consistent with H2b.

Immediately following the overall credibility question, Participants rated how objective Precision's management was in creating the financial statements and the letter on 11-point scales, with endpoints labeled "not at all objective/very objective." If participants' frame of reference was the audited financial statements, then participants in the WEB—with links

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[10] I examined the effect of extreme values in two ways. First, I created box plots for the variables used in each test. There were no extreme values. Second, I computed Cook's distance, which measures how much the coefficients change when any particular observation is deleted. Cook and Weisberg (1982) suggest a value greater than 1 deserves closer scrutiny. Across all tests, the highest Cook's value was 0.23, which suggests that no single observation significantly influences the reported results. Thus, I conclude that outliers are not driving the reported results.

[11] The two excluded pieces of information were (1) a discussion of navigation and guidance equipment, and (2) a debt-to-equity ratio above the industry average. Of the seven remaining pieces of information, four were from the financial statements and related footnotes (a discussion of revenue recognition, industry ratios, a discussion of inventory valuation, and a dividend payment), and three were from the letter/*pro forma* statements (a quick ratio equal to the industry average, a discussion of new products, and a discussion of computer equipment).

TABLE 1
Participants' Classification Error Rates, Credibility Assessments, and Earnings Potential Judgments

Panel A: Descriptive Statistics: Mean [standard deviation]

	Dependent Variable		
Presentation Condition	*Classification Error Rates*[a]	*Credibility*[b]	*Earnings Potential*[b]
Web with links (n = 18)[c]		6.47 [1.81]	6.17 [1.58]
Unaudited error rate	37.0% [32.1%]		
Audited error rate	22.2% [16.9%]		
Overall error rate	28.6% [18.3%]		
Web with aid (n = 13)		4.69 [1.97]	5.00 [1.53]
Unaudited error rate	33.3% [30.4%]		
Audited error rate	13.5% [16.5%]		
Overall error rate	22.0% [15.0%]		
Hardcopy no links (n = 16)[d]		5.13 [1.65]	5.56 [1.14]
Unaudited error rate	22.2% [16.3%]		
Audited error rate	15.0% [22.8%]		
Overall error rate	18.1% [13.7%]		

Panel B: Paired-Sample Comparison of Classification Error Rates within the WEB—with Links Condition

	df	*t-statistic*	*p-value*[e]
H1a: Unaudited error rate (37.0%) vs. Audited error rate (22.2%)	17	1.90	0.038

Panel C: Planned Comparison of Unaudited Classification Error Rates

	df	*t-statistic*	*p-value*[e]
H1b: WEB—with links (37.0%) vs. HARDCOPY—no links (22.2%)	31	1.62	0.058

Panel D: Correlation Analysis

	Pearson Correlation		
	n	*r*	*p-value*[e]
H2a: Unaudited error rate and credibility	45	0.25	0.047

Panel E: Planned Comparisons of Credibility Assessments

	df	*t-statistic*	*p-value*[e]
H2b: WEB—with links (6.47) vs. HARDCOPY—no links (5.13)	31	2.23	0.017

Panel F: Correlation Analysis

	Pearson Correlation		
	n	*r*	*p-value*[e]
H3a: Credibility and earnings potential	46	0.25	0.048

(continued on next page)

TABLE 1 (Continued)

Panel G: Planned Comparison of Earnings Potential Judgments

	df	*t-statistic*	*p-value*[e]
H3b: WEB—with links (6.17) vs. HARDCOPY—no links (5.56)	32	1.26	0.108

[a] In the classification task, participants identified whether each of seven pieces of information originated from the financial statements or the letter to shareholders. Overall error rate is the percentage of audited and unaudited pieces of information a participant misclassified. Unaudited error rate is the percentage of items from the letter to shareholders that a participant misclassified and audited error rate is the percentage of items from the financial statements that a participant misclassified.

[b] Participants provided their credibility assessments and earnings potential judgments using 11-point scales with endpoints labeled "not credible/very credible" and "very low/very high," respectively.

[c] Of the 18 participants in the WEB—with links condition, only 17 answered the credibility measure.

[d] Of the 16 participants in the HARDCOPY—no links condition, only 15 completed the classification task.

[e] p-values are one-tailed.

and HARDCOPY—no links conditions should perceive management as equally objective in creating the statements. If a credibility inflation effect exists, then participants in the WEB—with links condition should perceive management as more objective in creating the letter than participants in the HARDCOPY—no links condition. Untabulated results reveal that participants in the WEB—with links condition believed that Precision's management was more objective in creating the unaudited letter than did participants in the HARDCOPY—no links condition ($t_{32} = 2.49$, $p = 0.009$, one-tailed). In contrast, participants in the WEB—with links and HARDCOPY—no links conditions did not have different beliefs about how objective management was in creating the audited financial statements ($t_{32} = 0.18$, $p = 0.855$, two-tailed). These results suggest that hyperlinking the two documents inflated the perceived credibility of the information in the unaudited letter, without impairing the credibility of the information in the financial statements.

Earnings Potential Judgments

Panel F of Table 1 reports evidence supporting H3a's prediction that the more credible participants believe the entire information set to be, the higher they will judge the firm's earnings potential ($r_{46} = 0.25$, $p = 0.048$, one-tailed).[12] Hypothesis 3b predicts that participants in the WEB—with links condition will judge the firm's earnings potential as higher than will participants in the HARDCOPY—no links condition. Panel G of Table 1 reports that the earnings-potential judgments of WEB—with links participants are slightly higher than those of HARDCOPY—no links participants (6.17 vs. 5.56, $t_{32} = 1.26$, $p = 0.108$, one-tailed). Hypothesis 3b is at best marginally supported.

Notification Aid

Hypotheses 4a through 4c predict that notifying participants with an "AUDITED/NOT AUDITED" label as they navigate between the audited and unaudited information will reduce misclassification, credibility inflation, and the resulting increase in assessments of a firm's earnings potential. To examine these predictions, I compare the responses of participants in the WEB—with aid condition to those in the WEB—with links condition. If the notification aid is effective, then participants in the WEB—with aid condition should misclassify fewer unaudited items as audited than will participants in the WEB—with links condition, and their credibility and earnings-potential judgments should be less than the credibility and earnings potential judgments of participants in the WEB—with links condition.

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[12] Consistent with the results of H3a, the more objective participants perceived the letter to be the higher they judged the firm's earnings potential ($r_{47} = 0.35$, $p = 0.007$, one-tailed). Furthermore, mediation analysis suggests participants' beliefs about how objective management was in creating the unaudited letter accounts for the relation between how participants received the information (web vs. hardcopy) and their judgments of the firm's earnings potential. Results using participants' overall credibility assessments as the mediating variable are inconclusive.

Table 2 reports that participants in the WEB—with aid condition did not misclassify significantly fewer items of unaudited information as audited than did participants in the WEB—with links condition (33 percent vs. 37 percent, $t_{29} = 0.32$, $p = 0.374$, one-tailed). These results do not support H4a. In contrast, untabulated results reveal that the notification aid was effective in helping participants correctly classify the audited information: participants in the WEB—with aid condition misclassified only 14 percent of the audited information as unaudited, whereas participants in the WEB—with links condition misclassified 22 percent of the audited information as unaudited ($t_{29} = 1.44$, $p = 0.081$, one-tailed).

Table 2 shows that participants in the WEB—with aid condition assessed credibility as significantly lower than did participants in the WEB—with links condition (4.69 vs. 6.47, $t_{28} = 2.57$, $p = 0.008$, one-tailed), consistent with H4b. Similarly, Table 2 reports that the earnings-potential judgments of participants in the WEB—with aid condition are significantly lower those of participants in the WEB—with links condition (5.00 vs. 6.17, $t_{29} = 2.06$, $p = 0.025$, one-tailed), consistent with H4c.

One possible explanation for why results support H4b and H4c, but not H4a, is that I necessarily limited the classification task to a small number of potentially relevant pieces of information. The pattern of results for H4a through H4c could result if the notification aid helped individuals correctly identify enough unaudited items to influence their subsequent judgments, but the classification task did not include these items. It is also possible that a stronger notification aid, such as the example presented in Exhibit 2, might have been more effective in reducing participants' classification errors.

TABLE 2
The Effect of an AUDITED/NOT AUDITED Notification Aid on Participants' Classification Error Rates, Credibility Assessments, and Earnings Potential Judgments

Planned Comparisons of:	*df*	*t-statistic*	*p-value*[a]
Unaudited Classification Error Rates			
H4a: WEB—with aid (33.3%) vs. WEB—with links (37.0%)	29	0.32	0.374
Credibility Assessments			
H4b: WEB—with aid (4.69) vs. WEB—with links (6.47)	28	2.57	0.008
Earnings Potential Judgments			
H4c: WEB—with aid (5.00) vs. WEB—with links (6.17)	29	2.06	0.025

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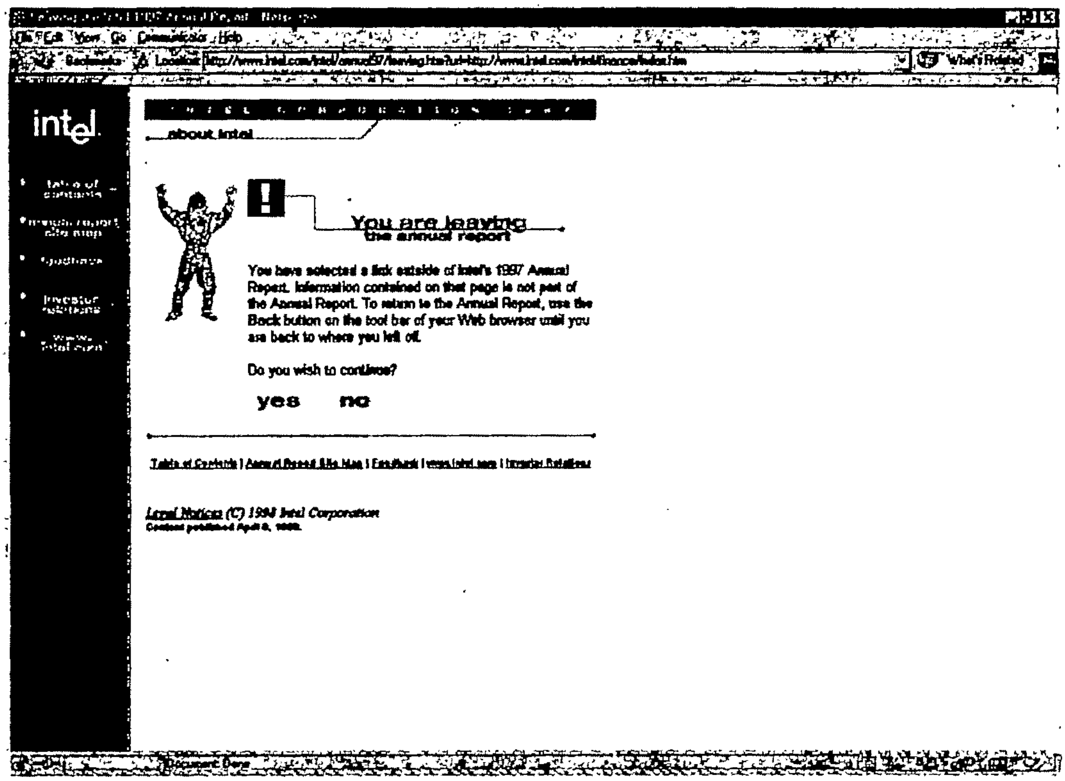
[a] p-values are one-tailed.

EXHIBIT 2
Intel Example



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V. CONCLUSIONS, IMPLICATIONS, AND LIMITATIONS

Eighty-five percent of all participants responded in the post-experiment questionnaire that their judgments would not depend on whether they viewed financial information on the Web or in hardcopy format. However, this study's empirical results contradict these beliefs and indicate that participants who viewed hyperlinked materials on the Web misclassified more optimistic, unaudited information as audited and believed the optimistic, unaudited information to be more credible than did participants who viewed hardcopy materials. Results also indicate that inflating the credibility of the optimistic unaudited information increased participants' judgments of the firm's earnings potential. Finally, results indicate that notifying web-based participants with an "AUDITED/NOT AUDITED" label as they navigated between the audited and unaudited information reduced their credibility assessments and their earnings-potential judgments.

The AICPA and the Canadian Institute of Chartered Accountants jointly developed a service called WebTrust, aimed at providing assurance for electronic commerce. My results suggest that a similar service aimed at providing assurance to financial-report users that a firm's web-based financial disclosures meet certain criteria, such as explicitly labeling, or prohibiting, direct links between audited and unaudited information, might benefit investors.

This study is subject to several limitations. First, I limit the amount of information participants receive to a subset of that available on most corporate web sites so that participants can complete the task in a reasonable amount of time. Most firms' web sites are more complex than my experimental setting, with multiple links between and within financial and nonfinancial information. Although this reduced complexity biases against my predictions, it also diminishes the generalizability of my findings. Future research could examine how the complexity of hyperlinked environments affects investor judgments.

Second, due to low sample size, this study does not disentangle how much of the effect of presenting hyperlinked information on the Web is due to presenting information on a computer screen vs. presenting it in hardcopy form, and how much is due to hyperlinking the information in a web-based environment.

Third, because 59 percent of participants completed the experiment in an "out-of-lab" setting, I cannot be completely sure that participants complied with my instructions to complete the case in one sitting and to refrain from looking back through the case materials when answering questions. Although I did not detect evidence that participants violated these instructions, my out-of-lab data-gathering technique cannot eliminate the possibility that uncontrolled, extraneous factors affected my results.

Fourth, I use graduate business students as surrogates for online investors. Although M.B.A. students exhibit demographic characteristics similar to those of online investors, many have limited investment experience and therefore may not reflect the beliefs, or use the analytical techniques, of actual investors. Despite these limitations, this study provides descriptive *ex ante* evidence to policymakers, auditors, and investors that firms can influence the perceptions of financial-report users by hyperlinking unaudited information to information in the audited financial statements, and that a practical, low-cost notification aid attenuates these influences.

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BOOK REVIEWS

Jane L. Reimers, Editor

Editor's Note: The book reviews in this issue show the wide spectrum of topics and issues that are available in accounting for our consideration and investigation. The first review deals with fraud auditing, a very current issue in our profession. The second deals with the history and development of accounting thought and takes us on a journey into the pursuit of accounting scholarship. Together, they bring a renewed appreciation of all that accounting has been and all that it can be.

DAVIA, HOWARD R., *Fraud 101: Techniques and Strategies for Detection* (New York, NY: John Wiley & Sons, Inc., 2000, 242 pp.).

Howard Davia's new book can change the way we teach auditors to detect fraud. This book makes two major contributions: the first is to define a realistic role for fraud auditors; the second is to give step-by-step procedures for detecting a variety of frauds.

Defining a role for fraud auditors is not an easy task. While SAS No. 82 mentions two types of fraud—one that results in *material misstatements on financial statements* and another that results in the *misappropriation of assets*—it makes clear that auditors are responsible only for discovering the first kind (but even that responsibility is qualified). While auditors take responsibility only for detecting frauds that result in material misstatements, audit clients want accounting firms to detect both types of frauds. Further, audit clients often assume that an unqualified audit opinion guarantees that they are free from both types of fraud. This is the infamous "expectations gap."

There are sound reasons for the accounting profession to avoid taking responsibility for asset-theft fraud. First, the cost of giving a complete fraud audit would be prohibitive to any client. Second, the accounting profession has not yet outlined procedures that would guarantee that a fraud audit would detect all manner of fraud. Thus, offering asset-fraud audits could open up accounting firms to even more liability than they currently face.

Davia's response is that accounting firms should not have to guarantee a firm is completely free of all types of asset-theft fraud. Rather, the accounting firms should offer a menu of specific types of frauds for which they would provide audits. Clients could then choose the extent of a fraud audit based on the amount of money they were willing to spend. The benefits would be two-fold: firms could be assured they are free of specific frauds, and potential criminals would be deterred by seeing the fraud auditors at work, never knowing which types of fraud they were investigating. Given the character-profile of white-collar criminals, this deterrence effect could be very effective.

Davia's book is written as a text for audit practitioners or students who want an introduction into detecting frauds. The approach is to teach the reader how to conduct "proactive" audits: going into a firm with a specific mission to search for frauds where no symptoms yet exist. The book is organized by broad fraud category. Each type of fraud is defined, examples of that fraud are given, and then audit procedures are given that would detect that fraud. The frauds for which he provides detection techniques in this book are: duplicate payment fraud, multiple payee fraud, shell fraud, defective delivery fraud, shipment or pricing fraud, and finally contract-related frauds. The book also contains sections on gratuities, bribes, kickbacks, fraud symptoms, and a small section on evidence. Davia includes some case studies and solutions, as well as interesting excerpts from government fraud hearings.

Many universities have added a forensic auditing component to their curriculum as a separate course or a block within an auditing course, but the coverage is not nearly as widespread as might be expected considering the demand for forensic auditors. Some of the reluctance may be the lack of textbooks that teach specific detection techniques. Davia's book begins to fill that gap. His book can be considered the second generation of fraud-auditing books. The first generation generally came from practitioners who became fraud specialists, not by classroom training, but by on-the-job experiences. Thus, their books were generally a collection of personal experiences. These books are useful in the sense that they raise fraud awareness among students. However, they are not organized to teach specific fraud-detection techniques.

Davia's book is appropriate as the text for an undergraduate class or as a single block within a special-topics class for graduate students. It does not attempt to be a comprehensive training course for forensic auditing, merely an introduction to proactive detection techniques. For the types of fraud he targets, he fulfills the book's stated mission.

For readers who want to go beyond the mere detection of elementary frauds, I recommend *The Accountant's Guide to Fraud Detection and Control*[1] a book that Davia co-authored. While the case studies in *Fraud 101* end at detection, after which students are counseled to turn the problem over to professional investigators, the *Accountant's Guide* cross-trains auditors in basic investigative techniques. The book includes instructions on interviewing skills, note taking, and dealing with prosecutors. The last section covers the rules of evidence and legal cases in fraud. In addition, there are sections on computer fraud, internal controls, and a large case study with solution.

While each book makes important contributions, the proactive techniques and the proposed role for forensic auditors given in *Fraud 101* deserve serious consideration. We should not expect to know all the procedures for detecting fraud at this time, but we can start building a body of procedures. After all, auditing itself began in 1939 with two simple procedures: observe physical inventory counts and confirm receivables. We should hope that Davia's book is the first of many books and articles that start refining fraud audit procedures.

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[1] Davia, H. R., P. C. Coggins, J. C. Wideman, and J. T. Kastantin 2000. *Accountant's Guide to Fraud Detection and Control.* 2nd edition. New York, NY: John Wiley & Sons, Inc.

CARL THOMAS DEVINE, *Essays in Accounting Theory: A Capstone,* Harvey Hendrickson, ed. (New York, NY: Garland Publishing, Inc., 1999, 328 pp.).

For some people of my (baby-boom) generation, the blurred mimeograph copies of Carl Devine's essays (later published as AAA Studies in Accounting Research No. 22) were what made us take seriously accounting as a worthwhile scholarly pursuit. There is a certain subversive quality to Devine's essays that comes largely from the remarkable erudition of the man who wrote them, standing in rather stark contrast to the lack of erudition of the people practicing what he wrote about. Like many others, I consider myself extremely fortunate to have had the opportunity to sit in Carl Devine's living room and listen to him range comfortably across many fields—art, music, literature, philosophy, physics, mathematics, biology—and relate what we learn from these disparate fields to what we accountants do. The present volume of essays will give those not so fortunate as me the opportunity to eavesdrop on those conversations and experience the intellectual exhilaration they engendered. Rereading the present volume of essays in preparation for this review was a very personal experience; some of the books Devine cites in the essays are now in my possession, filled with his scribbles along the margins. They are emblematic of Devine's generosity in helping others on their own personal journeys with accounting theory.

In an earlier, unedited version of this book given to me by Carl Devine, the dedication reads, in part, "These odds and ends may be of some interest." Indeed they are. This book contains 13 essays (two of which are in two parts). Rather than a capstone, as the title contends, they are a continuation of the work of one of our most thoughtful and imaginative scholars. The essays in this volume, like their predecessors, are an excursion through the mind of a quintessentially American intellectual who devoted his considerable scholarly talents to the seemingly mundane and unproblematic field of accounting. This volume of essays is a valuable read as a demonstration of how a genuinely scholarly mind works. Certainly every student of accounting bound over to the confines of methodology will find liberating Devine's imagination and playfulness with ideas most regard as foreign to accounting.

Among Devine's varied characterizations of himself was that he was "a bit of a grasshopper." That will be evident both between and within these essays. They jump around a bit and often contain extensive digressions before they return to their putative theme. If one prefers their accounting reading to arrive at an "interpretation of the results" by unswerving adherence to the stations of the cross—theory, hypothesis, model, data gathering—then one will be a bit discomfited by these essays. If there is a common theme to the essays, then it is probably that of addressing important developments in contemporary accounting scholarship. The two most notable are the Friedmanesque brand of positivism that tends to prevail in the U.S. academy and the "alternative" literature that gives more acknowledgement to the rhetorical turn in recent years in both the social and natural sciences. It may be that the motivation for many of the essays came from conversations and debates he had with Ed Arrington, another intellectual vagabond made welcome in the Devine home. In the essays, Devine confronts the present theory debates in the academy through an understanding of their history and his own painstakingly developed beliefs about the most fruitful ways to understand accounting.

In the first essay, titled "Accounting Theory: A Personal Journey," Devine elaborates upon his general philosophy of accounting; it is a frank disclosure of his foundation beliefs. He uses this essay to reiterate his commitments to subjectivism, pragmatism, behavioral science, ethics, and the significance of language to an understanding of human behavior. In this essay Devine remarks:

> It is still my stubborn belief that much of accounting cannot yet be supported by the endless statistical tables and Greek-letter operations favored by some editors of current journals. I remain genuinely disappointed with the continuing lack of attention to human interpretation and the necessity for value assessments and judgements. Meanwhile, for empiricists and their positivistic brothers the current journalistic *Zeitgeist* is favorable and the planetary orbits are in proper conjunction. (p. 16)

Indeed the remaining essays provide justifications for that stubborn belief and arguments as to why we might come to share his disappointment.

The second essay is a discussion of hermeneutics and deconstruction and what hermeneutists and deconstructionists have to contribute to our understanding of accounting. Particular attention is paid to the inescapable choice of a host group, i.e., for which groups does accounting produce its texts and has it paid careful enough attention to that choice? As Devine notes:

> My own discomfort with the accounting profession is more moderate but no less shrill. A system that neglects all values not related to scarcity is badly in need of help from both humanists and religious leaders. The judgement that only financial equity holders can become acceptable host groups is monstrous. Unbridled concern with the accountant's "bottom line" is simply obscene. (p. 42)

There follows some brilliant stuff about the reconsideration of what "equity" means and also "asset," which anticipates the current anxiety over the disconnect between market value and book value for many "technology" firms because of intangible assets not recognized.

Essay three is one of the two-part essays, the first part of which is generally supportive commentary on the arguments presented by Arrington and Francis in "Letting the Chat Out of the Bag: Deconstruction, Privilege and Accounting Research" (*Accounting, Organizations and Society* 1989, 1/2, 1–28). The second part contains a discussion of the literature of the absurd and its relationship to the problem of allocations, EMH, and other debates in accounting. It would be a great reading assignment for a theory class to help the student blow the dust off their brains.

The second two-part essay is number four. In Devine's words, "The objective of this essay is to demonstrate that adherence to a strict factual framework is not only undesirable but is next to impossible." The first part is a general consideration of positivism; the second part addresses the accounting positivists. Much of the critiques of positivism as a philosophy of accounting science tend to take on a rather strident tone. This is not just one more reiteration of the obvious. It is a balanced critique of positivism and the accounting positivists in that he gives their due by discussing the possible useful extensions of the principal-agent model in accounting.

Essays five, six, and seven are all concerned with "belief." In essay five, Devine provides his thoughts on the general problem of fixation of belief, which relates to the questions of evidence and methods of persuasion. He discusses the importance of discourse and correspondence and coherence theories about "truth." The essay also contains a brief exegesis on pragmatism with particular attention to Charles Pierce and William James.

Essay six is devoted to "accounting belief" and the role of authorities in establishing a basis for belief. This essay provides an excellent synopsis of the contributions of many of accounting's early theorists, e.g., J. M. Clark, William Vatter, William Paton, Edwards and Bell, etc. Devine also provides an excellent discussion of coherence and correspondence theories of "truth" in accounting and how they seem to apply to the work of the various early theorists. For graduate students who have little knowledge of the historical debates in accounting, which still persist in different epistemological guises, this essay is an excellent little history lesson, a lesson Devine believed was useful. He expressed disdain for those who do not understand from whence come ideas. In his words, "[i]nterestingly, this concern with recency has been adopted by many current research scholars who more or less disregard the history of ideas, quote only recent references, and throw away all journals of the previous decade" (p. 132).

Essay seven is concerned with auditing as a system of belief. The essay discusses auditors' three serious problem areas: (1) that of being at times advocates and at other times "independent" (a condition Devine thought was not possible); (2) that of auditors' ability to give opinions on managerial representations without themselves being values experts; and (3) that of setting guidelines for giving judgments on other people's representations about their intentions and expectations. This essay is particularly insightful given the current assertions about the dramatically expanded realm of "auditor" expertise.

The eighth essay is a brief discursion on the potential value of sociological models to accounting. Given the nature of accounting, it is somewhat surprising that sociology has remained relatively untapped in the U.S. as a field for analogs for accountants. This is especially true given how analogizing from economics and, to a lesser extent, cognitive psychology has been the predominant means of scholarly innovation in accounting over the last two or three decades. This essay provides encouragement to any accounting scholar who might like to venture into less traveled social science territory.

The next three essays are related in that they are concerned with the history of accounting "paradigms" and methodologies. Essay nine is devoted to the Kuhnian characterization about change in scientific fields and a look at some of the "paradigms" in accounting that died, e.g., D. R. Scott's institutionalist approach, Vatter's fund theory, and the price-level controversy. He also provides his assessments of capital market theory and principal-agent theory as paradigms. Essay ten is devoted to income theory with particular emphasis on the contributions of Irving Fisher. Essay 11 revisits some of the accounting leaders from the past and the "paradigms" they represented. A portion of this essay is devoted to a discussion of a problem that is a recurring theme in much of Devine's thinking, that of selecting a host. Poignantly, he concludes by observing that it is unfortunate that embezzlers, money launderers, narcotic tradesmen, and the:

> Ivan Boeskys of high finance usually have more than adequate resources and often know which pressure centers are subject to influence. They create their own symphony of squeaks, but it is not obvious that a service profession should listen to them. Mastering the strategies of market finance may create canny and effective operators, but it is doubtful that such skills will contribute to an effective service profession. (p. 261)

The penultimate essay, number 12, is a bit whimsical, as he reminisces about accounting personalities he knew over his 50 or so years in the academy. These are historically valuable, and it is fortunate that Devine put these to paper so that they become a permanent part of the archive of accounting history. Devine has a delightfully candid way of writing about these people. For example, about H. A. Finney, he says, "My personal experiences with H. A. Finney were limited and usually accompanied with generous portions of Johnny Walker, and as a result these observations may be of minor interest" (p. 283).

The final essay begins with discussion of the outmoded doctrine of "ccst attachment" and its historical origins. This segues into a discussion of social costs and then into socially necessary costs and then into a digression on Marxism and the New Left before, in grasshopper fashion, returning to the theme of analogizing accounting to the practice of science.

Carl Devine passed away in 1998 and, sadly, Harvey Hendrickson, who painstakingly edited these essays for publication, passed away even while I was preparing this review. This volume of essays is a testament to two men who each decided that, in Devine's words, "Being a bookworm is an honorable way to spend one's life." As a history of ideas, as constructive critique of theory past and present, as a source of insight and understanding of contemporary accounting, and as exemplar of genuine scholarship, I highly recommend this book.

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Editorial Data

The following table contains information about turnaround time for manuscripts (including revisions) on which editorial decisions were made in the 12-month period ended May 31, 2001. Turnaround time is the number of days between the date that the manuscript was received *and the date of the editor's letter to the author(s).*

	Number of Manuscripts	Cumulative Number	Cumulative Percent
$0 \leq$ Days ≤ 30	79	79	25.40
$31 \leq$ Days ≤ 60	104	183	58.84
$61 \leq$ Days ≤ 90	82	265	85.21
$91 \leq$ Days ≤ 120	33	298	95.82
$121 \leq$ Days	13	311	100.00

The mean review time was 55 days; the median review time was 54 days.

EDITORIAL POLICY AND STYLE INFORMATION

EDITORIAL POLICY

According to the policies set by the Publications Committee (which were endorsed by the Executive Committee and were published in the *Accounting Education News*, June 1987), *The Accounting Review* "should be viewed as the premier journal for publishing articles reporting the results of accounting research and explaining and illustrating related research methodology. The scope of acceptable articles should embrace any research methodology and any accounting-related subject, as long as the articles meet the standards established for publication in the journal....No special sections should be necessary. The primary, but not exclusive, audience should be—as it is now—academicians, graduate students, and others interested in accounting research."

The primary criterion for publication in *The Accounting Review* is the significance of the contribution an article makes to the literature.

The efficiency and effectiveness of the editorial review process is critically dependent upon the actions of both authors submitting papers and the reviewers. Authors accept the responsibility of preparing research papers at a level suitable for evaluation by independent reviewers. Such preparation, therefore, should include subjecting the manuscript to critique by colleagues and others and revising it accordingly prior to submission. The review process is not to be used as a means of obtaining feedback at early stages of developing the research.

Reviewers and associate editors are responsible for providing critically constructive and prompt evaluations of submitted research papers based on the significance of their contribution and on the rigor of analysis and presentation. Associate editors also make editorial recommendations to the editor.

MANUSCRIPT PREPARATION AND STYLE

The Accounting Review's manuscript preparation guidelines follow (with a sight modification) the B-format of *The Chicago Manual of Style* (14th ed.; University of Chicago Press). Another helpful guide to usage and style is *The Elements of Style*, by William Strunk, Jr., and E. B. White (Macmillan). Spelling follows *Webster's International Dictionary*.

FORMAT

1. All manuscripts should be typed in 12-point font on one side of $8\frac{1}{2} \times 11''$ good quality paper and be double spaced, except for indented quotations.
2. Manuscripts should be as concise as the subject and research method permit, generally not to exceed 7,000 words.
3. Margins of at least one inch from top, bottom, and sides should facilitate editing and duplication.
4. To promote anonymous review, authors should not identify themselves directly or indirectly in their papers or in experimental test instruments included with the submission. Single authors should not use the editorial "we."
5. A cover page should show the title of the paper, the author's name, title and affiliation, email address, any acknowledgments, and a footnote indicating whether the author would be willing to share the data (see last paragraph in this statement).

Pagination: All pages, including tables, appendices and references, should be serially numbered. Major sections should be numbered in Roman numerals. Subsections should not be numbered.

Numbers: Spell out numbers from one to ten, except when used in tables and lists, and when used with mathematical, statistical, scientific, or technical units and quantities, such as distances, weights and measures. For example: *three days; 3 kilometers; 30 years.* All other numbers are expressed numerically.

Percentages and Decimal Fractions: In nontechnical copy use the word percent in the text.

Hyphens: Use a hyphen to join unit modifiers or to clarify usage. For example: *a well-presented analysis; re-form.* See *Webster's* for correct usage.

Keywords: The abstract is to be followed by four keywords that will assist in indexing the paper.

ABSTRACT/INTRODUCTION

An Abstract of about 100 words should be presented on a separate page immediately preceding the text. The Abstract should concisely inform the reader of the manuscript's topic, its methods, and its findings.

Keywords and the Data Availability statements should follow the Abstract. The text of the paper should start with a section labeled "I. Introduction," which provides more details about the paper's purpose, motivation, methodology, and findings. Both the Abstract and the introduction should be relatively nontechnical, yet clear enough for an informed reader to understand the manuscript's contribution. The manuscript's title, but neither the author's name nor other identification designations, should appear on the Abstract page.

TABLES AND FIGURES

The author should note the following general requirements:

1. Each table and figure (graphic) should appear on a separate page and should be placed at the end of the text. Each should bear an Arabic number and a complete title indicating the exact contents of the table or figure. Tables and figures should define each variable. The titles and definitions should be sufficiently detailed to enable the reader to interpret the tables and figures without reference to the text.
2. A reference to each graphic should be made in the text.
3. The author should indicate by marginal notation where each graphic should be inserted in the text.
4. Graphics should be reasonably interpreted without reference to the text.
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Equations: Equations should be numbered in parentheses flush with the right-hand margin.

DOCUMENTATION

Citations: Work cited should use the "author-date system" keyed to a list of works in the reference list (see below). Authors should make an effort to include the relevant page numbers in the cited works.

1. In the text, works are cited as follows: authors' last name and date, without comma, in parentheses: for example, (Jones 1987); with two authors: (Jones and Freeman 1973); with more than two: (Jones et al. 1985); with more than one source cited together (Jones 1987; Freeman 1986); with two or more works by one author: (Jones 1985, 1987).
2. Unless confusion would result, do not use "p." or "pp." before page numbers: for example, (Jones 1987, 115).
3. When the reference list contains more than one work of an author published in the same year, the suffix a, b, etc. follows the date in the text citation: for example, (Jones 1987a) or (Jones 1987a; Freeman 1985b).
4. If an author's name is mentioned in the text, it need not be repeated in the citation; for example, "Jones (1987, 115) says...."
5. Citations to institutional works should use acronyms or short titles where practicable; for example, (AAA ASOBAT 1966); (AICPA Cohen Commission Report 1977). Where brief, the full title of an institutional work might be shown in a citation: for example, (ICAEW *The Corporate Report* 1975).
6. If the manuscript refers to statutes, legal treatises or court cases, citations acceptable in law reviews should be used.

Reference List: Every manuscript must include a list of references containing only those works cited. Each entry should contain all data necessary for unambiguous identification. With the author-date system, use the following format recommended by *The Chicago Manual*:

1. Arrange citations in alphabetical order according to surname of the first author or the name of the institution responsible for the citation.
2. Use author's initials instead of proper names.
3. Date of publication should be placed immediately after author's name.
4. Titles of journals should not be abbreviated.
5. Multiple works by the same author(s) in the same year are distinguished by letters after the date.
6. Inclusive page numbers are treated as recommended in *Chicago Manual* section 8.67.

Sample entries are as follows:

American Accounting Association, Committee on Concepts and Standards for External Financial Reports. 1977. *Statement on Accounting Theory and Theory Acceptance*. Sarasota, FL: AAA.

Demski, J. S., and D. E. M. Sappington. 1989. Hierarchical structure and responsibility accounting. *Journal of Accounting Research* 27 (Spring): 40–58.

Dye, R., B. Balachandran, and R. Magee. 1989. Contingent fees for audit firms. Working paper, Northwestern University, Evanston, IL.

Fabozzi, F., and I. Pollack, eds. 1987. *The Handbook of Fixed Income Securities*. 2nd edition. Homewood, IL: Dow Jones-Irwin.

Kahneman, D., P. Slovic, and A. Tversky, eds. 1982. *Judgment Under Uncertainty: Heuristics and Biases*. Cambridge, U.K.: Cambridge University Press.

Porcano, T. M. 1984a. Distributive justice and tax policy. *The Accounting Review* 59 (October): 619–636.

———. 1984b. The perceived effects of tax policy on corporate investment intentions. *The Journal of the American Taxation Association* 6 (Fall): 7–19.

Shaw, W. H. 1985. Empirical evidence on the market impact of the safe harbor leasing law. Ph.D. dissertation, The University of Texas at Austin.

Sherman, T. M., ed. 1984. *Conceptual Framework for Financial Accounting*. Cambridge, MA: Harvard Business School.

Footnotes: Footnotes are not used for documentation. Textual footnotes should be used only for extensions and useful excursions of information that if included in the body of the text might disrupt its continuity. Footnotes should be consecutively numbered throughout the manuscript with superscript Arabic numerals. Footnote text should be doubled-spaced and placed at the end of the article.

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Authors should note the following guidelines for submitting manuscripts:

1. Manuscripts currently under consideration by another journal or publisher should not be submitted. The author must state that the work is not submitted or published elsewhere.
2. In the case of manuscripts reporting on field surveys or experiments, four copies of the instrument (questionnaire, case, interview plan or the like) should be submitted. Information that might identify the author(s) must be deleted from the instrument.
3. Four copies should be submitted together with a check in U.S. funds for $75.00 for members or $100.00 for nonmembers of the AAA made payable to the American Accounting Association. Effective January 1990, the submission fee is nonrefundable.
4. The author should retain a copy of the paper.
5. Revisions must be submitted within 12 months from request, otherwise they will be considered new submissions.

COMMENTS

Comments on articles previously published in *The Accounting Review* will be reviewed (anonymously) by two reviewers in sequence. The first reviewer will be the author of the original article being subjected to critique. If substance permits, a suitably revised comment will be sent to a second reviewer to determine its publishability in *The Accounting Review*. If a comment is accepted for publication, the original author will be invited to reply. All other editorial requirements, as enumerated above, also apply to proposed comments.

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OCTOBER 2001 PLACEMENT ADS

*The deadline for information to be included in the "Placement Information" section is two months prior to the date of publication. The **Review** is published on the first day of January, April, July, and October. Placement advertising and replies should be sent to the American Accounting Association, 5717 Bessie Drive, Sarasota, FL 34233-2399, ATTENTION: ADVERTISING DEPT. All replies to box numbers should be on separate sheets to facilitate forwarding.*

401 UNIVERSITY OF NOTRE DAME, Department of Accountancy, invites applications for position openings at the Assistant and Associate Professor levels. All research areas will be considered. Applicants for Assistant Professor positions should expect to complete doctoral degree requirements before the start of the contract period. All candidates should have a strong commitment to scholarly research and excellence in teaching. Salary is very competitive and research support is excellent. Please send *curriculum vitae* and an example of scholarly work to Professor Tom Schaefer, Chair, Department of Accountancy, Room 102D, Mendoza College of Business, University of Notre Dame, Notre Dame, IN 46556-5646. The University of Notre Dame is an Equal Opportunity/Affirmative Action Employer.

402 LOUISIANA STATE UNIVERSITY, Department of Accounting in the E. J. Ourso College of Business Administration, invites applications for one or more anticipated tenure-track openings to begin August 2002. Applications at the Assistant and Associate rank will be considered in all disciplines. Applications at the Associate or Full rank will be considered with a specialty in Auditing, Accounting Information Systems, or Financial Accounting. Required Qualifications (Associate/Full Professor): Ph.D. in Accounting or related field; Required Qualifications (Assistant Professor): Doctoral degree in Accounting be completed, or near completion, by the date of appointment (if degree is not completed by date of appointment, rank will be at Instructor level). Additional Qualification Desired (Assistant/Associate/Full Professor): CPA. Faculty are expected to teach undergraduate and graduate courses, be actively involved in academic research, and provide service to the profession and the university. Applications will be accepted until November 1 and will continue until candidates are selected. Please send a letter of application, *curriculum vitae*, and references to Don W. Finn, Chair, Department of Accounting, 3101 CEBA Building, Louisiana State University, Ref: Log #1234, Baton Rouge, LA 70803. LSU is an Equal Opportunity/Equal Access Employer.

403 BOSTON COLLEGE, Wallace E. Carroll School of Management, invites applications for a tenure-track Accounting position for Fall 2002. The qualified candidate must demonstrate interest in and capacity to conduct high-quality scholarly research as well as a high level of teaching competence and innovation. We seek individuals with an openness to diverse intellectual outlooks and research methodologies and an appreciation for Boston College's liberal arts tradition. We offer a collegial environment and an energetic faculty with a variety of intellectual interests and close ties to the Boston financial and academic communities. Direct inquiries to Gregory Trompeter, Chair, Department of Accounting, Carroll School of Management, Boston College, Chestnut Hill, MA 02467. Deadline for applications is January 22, 2002. The Carroll School of Management is an Equal Opportunity Employer and strongly encourages applications from individuals who will enhance the diversity of our faculty.

404 RADFORD UNIVERSITY invites applications for a tenure-track position at the Assistant Professor rank beginning Fall 2002. Area of expertise open. Ability to teach Accounting Information Systems a definite plus. Graduate teaching in the M.B.A. program expected. Ph.D./D.B.A. in Accounting required. ABDs with firm completion dates will be considered. Professional certification highly desirable. The Department of Accounting and Finance is part of the College of Business and Economics that offers AACSB-accredited programs. Radford University with an enrollment of approximately 8,700 students is a comprehensive state-supported institution located in scenic southwestern Virginia. Radford emphasizes teaching. Visit our web site at http://www.radford.edu. Salary and benefits are competitive. Review of applications begins in September 2001. Send letter of interest, current *curriculum vitae*, and names, addresses, and phone numbers of three references to Dr. Felix E. Amenkhienan, Chair, Accounting and Finance, Box 6951, Radford University, Radford, VA 24142; Email: famenkhi@radford.edu. Radford University is an Equal Opportunity/Affirmative Action Employer. Minorities and women are encouraged to apply.

405 UNIVERSITY OF TENNESSEE, KNOXVILLE, is accepting applications for one full-time, tenure-track position (Assistant or Associate Professor) to begin August 1, 2002. Applicants should have a primary interest in Financial Accounting, an interest in M.B.A. education and technology applications, and relevant professional experience. A candidate for Assistant Professor should hold a doctoral degree in Accounting or expect to complete all degree requirements by Fall 2002. The candidate should demonstrate excellent teaching and research potential. A candidate for Associate Professor should hold a doctoral degree in Accounting and should demonstrate excellent achievement in teaching and research. All candidates should demonstrate a strong appreciation for issues pertaining to business and accounting practice. Applications from minorities and women are strongly encouraged. Review of applications will continue until position is filled. Send applications to Dr. Keith G. Stanga, Head; Department of Accounting and Business Law; CBA; University of Tennessee; Knoxville, Tennessee 37996-0560. UT, Knoxville is an EEO/AA/Title VI /Title IX/Section 504/ADA/ADEA institution in the provision of its education and employment programs and services.

406 BOISE STATE UNIVERSITY, Department of Accountancy, is seeking applicants for a tenure-track position at the Assistant rank starting Fall 2002. Preference will be given to applicants with expertise in Cost/Managerial. Applicants with Auditing and/or Systems expertise will also be considered. A Ph.D. in Accounting is required (ABDs will be considered). Professional experience and certification are desired. Expectations include a commitment to high-quality teaching, scholarship leading to refereed publication, and university and community service. The department is AACSB-accredited and currently offers both undergraduate and master's degrees. The department has 14 full-time faculty and graduates approximately 100 students annually. Salary is competitive in the western region. Candidates should send a letter of interest, resume, and three letters of reference to Dr. Paul Bahnson, Department of Accountancy, Boise State University, Boise, ID, 83725-1610. The closing date for applications is December 1, 2001. Boise State University is an EEO/AA employer.

407 ITHACA COLLEGE, School of Business, invites applications for a tenure-track position in Accounting beginning August 2002. Primary responsibilities include teaching Cost and possibly Financial and Managerial Accounting to Accounting and Business majors. The School of Business offers undergraduate programs in Accounting and Management and a one-year M.B.A. program. Candidates should hold a Ph.D. or ABD in Accounting. Rank is open. Salary is competitive. Achieving tenure at Ithaca College is dependent on evidence of excellence in teaching, scholarship, and service. The School is in AACSB-candidacy status; publications are expected. Review of applications will begin immediately and will continue until the position is filled. Applicants should send a letter of application, *curriculum vitae*, and names of three references to Hugh C. Rowland, Associate Dean, School of Business, Ithaca College, Ithaca, NY 14850; Fax: (607) 274-1152; Phone: (607) 274-3940; Email: rowlandh@ithaca.edu. Ithaca College is an Equal Opportunity/Affirmative Action institution.

408 ITHACA COLLEGE, School of Business, invites applications for a tenure-track position in Accounting beginning August 2002. Primary responsibilities include teaching Auditing and possibly Advanced, Financial, and Managerial Accounting to Accounting and Business majors. The School of Business offers undergraduate programs in Accounting and Management and a one-year M.B.A. program. Candidates should hold a Ph.D. or ABD in Accounting. Rank is open. Salary is competitive. Achieving tenure at Ithaca College is dependent on evidence of excellence in teaching, scholarship, and service. The School is in AACSB-candidacy status; publications are expected. Review of applications will begin immediately and will continue until the position is filled. Applicants should send a letter of application, *curriculum vitae,* and names of three references to Hugh C. Rowland, Associate Dean, School of Business, Ithaca College, Ithaca, NY 14850; Fax: (607) 274-1152; Phone: (607) 274-3940; Email: rowlandh@ithaca.edu. Ithaca College is an Equal Opportunity/Affirmative Action institution.

409 YORK UNIVERSITY, Schulich School of Business, Toronto, Canada, is seeking tenure-stream faculty in Accounting at the Assistant, Associate, or Full Professor level effective July 1, 2002. Preferred candidates will (1) be qualified to teach in Financial or Managerial Accounting—expertise teaching International Accounting would be beneficial; (2) possess a completed doctorate or be close to completion; (3) provide evidence of effective teaching; (4) have a commitment to scholarly research and publication; (5) be willing to use cases in teaching. Please email application letter and *curriculum vitae* to Professor Linda Thorne, Accounting Area Coordinator: lthorne@schulich.yorku.ca. Salary and benefits are competitive. This position is subject to budgetary approval. York University has a policy of employment equity, including affirmative action for women, racial/visible minorities, persons with disabilities and aboriginal peoples. Persons who are members of one or more of these groups are encouraged to self-identify during the selection process. Please note that candidates from these four groups will be considered within the priorities of the Affirmative Action Program only if they self-identify. The Schulich School of Business welcomes applications from these groups. The Affirmative Action Program can be found on York's web site at http://www.yorku.ca.

410 UNIVERSITY OF LOUISIANA AT LAFAYETTE invites applications for a tenure-track position in Accounting at the rank of Assistant/Associate Professor, depending on qualifications, effective August 2001. The applicant must possess an earned doctorate in Accounting with primary teaching interests and experience in one or more of the following areas: Tax, Financial Accounting, or Accounting Information Systems. Candidates also should have an established record or demonstrated potential for excellence in teaching and research. Industry experience would be welcome. Salary will be competitive, depending upon qualifications and experience. Screening of applications will begin immediately and will continue until the positions are filled. Candidates are invited to send a letter of application, current *curriculum vitae* and transcript, and three letters of reference to Head, Search Committee—Accounting Positions, College of Business Administration, University of Louisiana at Lafayette, PO Box 40200, Lafayette, LA 70504-0200. The University of Louisiana at Lafayette is an Affirmative Action/Equal Opportunity Employer.

411 MASSACHUSETTS INSTITUTE OF TECHNOLOGY, Sloan School of Management's Accounting Group, seeks new faculty at all levels (i.e., Assistant, Associate, and Full Professor) for the academic year beginning July 1, 2002. Applicants will be judged on their potential for excellence in research and teaching. Applicants should submit a current *curriculum vitae,* evidence of research such as publication, working papers, or dissertation proposal and book chapters, and letter(s) of reference to Professor S. P. Kothari, Chairperson of Search Committee, MIT Sloan School of Management, 50 Memorial Dr., E52-325, Cambridge, MA 02142-1347. The deadline for applications is January 15, 2002. However, applications will be processed as soon as received if submitted at an earlier date. MIT is an Equal Opportunity Employer and encourages applications from women and minorities.

412 UNIVERSITY OF NEW MEXICO seeks candidates for a tenure-tenure track Assistant/Associate position for Fall 2002, subject to budget considerations. For best consideration, please submit by 8/1/2001. Ph.D./D.B.A. in Accounting or related field is required. ABDs are to be completed by commencement of tenure-track appointment. Applicants must be prepared to teach at graduate and undergraduate levels. Desirable qualifications include evidence of an ongoing program of research or the potential to develop such a program; compatibility with current department research thrusts; evidence of strong teaching credentials, and all areas of Accounting will be considered with preference given to teaching experience in Accounting Information Systems. Send letter of application; *curriculum vitae*; sample of one published work or working paper and/or dissertation proposal; teaching evaluations; and names, addresses of three references to Dr. Alistair Preston, UNM, Anderson Schools, 1924 Las Lomas NE, Albuquerque NM 87131; Email: Alistair@mgt.unm.edu. UNM is an EEO/AA Employer and Educator.

413 VILLANOVA UNIVERSITY invites applicants for a tenure-track position effective Summer 2002 (or Fall 2002 at the latest), to teach primarily in our graduate programs, including our new master's program in Accounting and Professional Consultancy. Experienced faculty, at all levels, with established records of research and teaching excellence are encouraged to apply. Applicants should possess at least four years of consulting or public accounting experience and a minimum of two years of full-time university-level teaching experience. Send application letter and *curriculum vitae*, and have three letters of recommendation sent, to James P. Borden, College of Commerce and Finance, Villanova University, Villanova, PA 19085-1678; Email: james.borden@villanova.edu. Villanova is a Roman Catholic university sponsored by the Augustinian Order. Review of applications begins July 1st and will continue until the position is filled. An AA/EEO Employer, we seek a diverse faculty committed to scholarship, service, and teaching, who understand, respect, and can contribute to the University's mission and values.

414 MASTER UNIVERSITY, Michael G. DeGroote School of Business (a "World's Top 100" Business School, *Financial Times* 2001 rankings), is seeking tenure-stream faculty in Accounting at the Assistant Professor level. We are looking to fill one position in July 1, 2002 and possibly one more in July 1, 2003, and offer competitive salary and benefits and an attractive teaching load. Applicants should have primary teaching and research interests in Financial/Managerial Accounting or Auditing. Preferred candidates will possess a completed doctorate or be close to completion; exhibit research excellence (or potential) and display teaching excellence (or effectiveness). For further information please email nainar@mcmaster.ca or visit http://www.business.mcmaster.ca. Please send a *curriculum vitae*, official transcripts, three letters of reference, teaching evaluations (if any), and a working paper to Professor S. M. Khalid Nainar, Chair, Accounting Area, Michael G. DeGroote School of Business, McMaster University, 1280 Main Street West, Hamilton, Ontario, L8S 4M4 CANADA.

415 LONG ISLAND UNIVERSITY, C.W. Post Campus, College of Management, School of Professional Accountancy, is seeking applications for a tenure-track position (rank open) beginning Fall 2002. Applicants must hold a Ph.D. or D.B.A. in Accounting (prior to appointment) and a valid CPA certificate. In addition to teaching, the faculty member will be expected to conduct and publish research and participate in service and professional activities. The College of Management is accredited by the AACSB and offers both undergraduate and graduate programs. Review of applications will begin September 15, 2001 and will continue until the position is filled. A letter of application, references, and a *curriculum vitae* should be sent in writing or electronically to Professor Robert Bertucelli, Chair, Personnel Committee, School of Professional Accountancy, Long Island University, C.W. Post Campus, 720 Northern Boulevard, Brookville, NY 11548-1300; Email: Robert.Bertucelli@liu.edu; Web: http://www.liu.edu/msacc. Long Island University is an Equal Opportunity Educator and Employer.

416 WESTERN MICHIGAN UNIVERSITY, Haworth College of Business, invites applications for a tenure-track position as Assistant Professor of Accounting beginning Fall 2002. Applicants must hold a doctoral degree in Accounting or IS or be ABD and near completion. The ideal candidates will have primary teaching and research interests in Accounting Information Systems and/or Taxation. Excellent English communication skills are required. Western Michigan University is a Carnegie-designated Doctoral/Research-Extensive University, and is only one of four Michigan public universities to be included in Carnegie's top research classification. Both the Accounting undergraduate and graduate programs are accredited by the AACSB. Please send *curriculum vitae* and research proposals and/or samples of research to Jack M. Ruhl, Chairperson, Department of Accountancy, Haworth College of Business, Western Michigan University, Kalamazoo, MI 49008. Western Michigan University is an Affirmative Action/Equal Opportunity Employer and has a strong commitment to diversity.

417 WIDENER UNIVERSITY, Department of Accounting and Taxation, invites applications for a tenure-track position at the rank of Assistant/Associate Professor, starting Fall 2002. Candidates' primary teaching interests should be in Accounting Information Systems. Interest in Auditing, or Managerial Accounting desirable. A Ph.D. or D.B.A., completed or in advanced stages, is required. Professional certification a plus. Candidates must have demonstrated potential for excellence in teaching and research, and will contribute in further developing an information technology-focused curriculum. The SBA, which is AACSB-accredited, offers a major in Accounting, and M.S. in Accounting Information Systems, M.S. in Taxation, and M.B.A. It is a member of the SAP University Alliance Program. The undergraduate Accounting and graduate AIS programs are Endorsed Internal Auditing Programs by the IIA. Send *curriculum vitae* to Dr. Joseph Hargadon, Widener University, One University Place, Chester, PA 19013; Phone: (610) 499-4280; Email: joseph.m.hargadon@widener.edu.

418 FLORIDA ATLANTIC UNIVERSITY has tenure-track positions beginning Fall 2002 at the Assistant Professor rank. Preference is for candidates with teaching interests in Tax, Managerial, or Financial Accounting, but all qualified candidates will be considered. Candidates should hold a doctoral degree in Accounting or expect to complete the degree by Fall 2002. They should be committed to excellent teaching and quality research. Professional certification is desirable. FAU is a public university with campuses in Boca Raton, Fort Lauderdale, Davie, Jupiter, and Port St. Lucie. It has over 20,000 students. The College of Business is AACSB-accredited and the School of Accounting has 23 faculty, offering degrees at the undergraduate, master's, and doctoral levels. Interested candidates should send their *curriculum vitae* to Dr. Carl Borgia, Director, School of Accounting, Florida Atlantic University, 777 Glades Road, Boca Raton, FL 33431; Fax: (561) 297-7023; Email: borgiac@Fau.edu.

419 UNIVERSITY OF UTAH, School of Accounting and Information Systems, is recruiting for three Assistant or Associate tenure track faculty members, to begin in the 2002–03 academic year. Applicants should possess a doctorate or be close to completion. We have special needs in Financial, Managerial, and Information Systems. Salaries, research support, and teaching loads are competitive and fringe benefits are excellent. The School of Accounting and Information Systems and the David Eccles School of Business are both accredited at the undergraduate and graduate levels by the AACSB. Bachelor's, M.B.A., Master's of Accounting, and Ph.D. programs are offered. The application deadline is February 1, 2002, or until the position is filled. Please send application, including dissertation proposal or working paper, *curriculum vitae*, three letters of reference and indication of teaching effectiveness, to Christine Botosan, 1645 E. Campus Center Drive, Rm. 108, University of Utah, Salt Lake City, UT 84112. The University of Utah is an EO/AA Employer and encourages applications from women and minorities and provides reasonable accommodations to the known disabilities of employees.

420 VIRGINIA POLYTECHNIC INSTITUTE & STATE UNIVERSITY (Virginia Tech), Pamplin College of Business, Department of Accounting and Information Systems, Blacksburg, Virginia, invites applicants for an Assistant Professor, tenure-track position starting August 10, 2002. Applicant's primary research and teaching interests should be in Accounting Information Systems. Successful candidate will instruct undergraduate and graduate students in Accounting Information Systems. Qualifications include Doctoral requirements completed and demonstrable prospects for conducting scholarly research and excellence in teaching. Applications accepted until October 15, 2001, or until position is filled. Send applications to Wayne Leininger, Department of Accounting and Information Systems, 3007 Pamplin (0101), Virginia Tech, Blacksburg, VA 24061. Virginia Tech has a strong commitment to the principle of diversity and, in that spirit, seeks a broad spectrum of candidates including women, minorities, and people with disabilities. Individuals with disabilities desiring accommodations in the application process should notify Wayne Leininger, (540) 231-6591 or TTY (540) 231-9460, by the application deadline.

421 UNIVERSITY OF SOUTH CAROLINA, School of Accounting, invites applicants for a tenure-track position for Fall 2002. Although we encourage applications from all ranks and areas of interest, our preference is for an Assistant Professor with a primary interest in Auditing. Applicants must possess a doctorate or be close to completion by August 2002. The School of Accounting has approximately 250 undergraduate majors (Juniors and Seniors) and 50 master's students and offers B.S., Master's of Accountancy, and Ph.D. degrees. Please submit a resume and a working paper or a defended dissertation proposal, if at the dissertation stage. Applications will be accepted until the position is filled. Submit applications to Professor Robert A. Leitch, School of Accounting, The Darla Moore School of Business, University of South Carolina, Columbia, SC 29208; Email: leitch@darla.badm.sc.edu. The University of South Carolina is an Affirmative Action/Equal Opportunity institution.

422 UNIVERSITY OF DELAWARE, Department of Accounting and MIS, is seeking an Information Systems faculty member capable of both quality research and teaching at the undergraduate and graduate levels. Ph.D. or D.B.A. in MIS or AIS is required. The University of Delaware, ranked 25th among the nation's top public universities by *U.S. News and World Report*, is located on a beautiful campus in Newark, Delaware. The Department of Accounting and MIS is part of the College of Business and Economics that serves about 1,700 undergraduate majors and approximately 670 graduate students. The undergraduate and graduate Accounting programs are AACSB-accredited. Representatives will be attending the AAA Annual Meeting in Atlanta. Candidates should send a *curriculum vitae*, three letters of reference, and summaries of teaching performance to Dr. Clinton White, Accounting and MIS, University of Delaware, Newark, DE 19716-2715; Email: SKIPW@udel.edu. Application deadline is November 15, 2001. The *curricula vitae* and letters of reference shall be shared with departmental faculty. The University of Delaware is an Equal Opportunity Employer that encourages applications from minority group members and women.

423 STATE UNIVERSITY OF NEW YORK AT BUFFALO (SUNYAB), School of Management, has two August 2002 openings for an Assistant, Associate, or Full Professor of Accounting. Candidates may have teaching and research interests in Financial or Managerial Accounting, Auditing, or Information Systems, and must have a demonstrated capacity to undertake scholarly research publishable in leading journals. Aptitude for quality teaching in M.B.A. and executive M.B.A. programs is preferred. Teaching opportunities are also available at undergraduate and Ph.D. levels. Doctorate in hand or (at the Assistant level) near completion from a reputable research university is expected. Research record is a requirement for senior positions. Salary and fringe benefits are competitive. Send resume, two letters of recommendation, copy of dissertation proposal and/or other research paper(s) to Professor Susan Hamlen, Department of Accounting and Law, Jacobs Management Center, SUNY at Buffalo, Buffalo, NY 14260-4000; Fax: (716) 645-3823; Email: hamlen@acsu.buffalo.edu. SUNYAB is an Affirmative Action/Equal Opportunity Employer.

424 UNIVERSITY OF WYOMING, Department of Accounting, is seeking applicants to fill two tenure-track positions (subject to approval) beginning in January 2002 or August 2002. Qualifications include a completed Ph.D. or D.B.A. in Accounting, with demonstrated potential for quality teaching and scholarship. Professional certification is highly desirable. Desired primary teaching specialties for the first position include Information Management/Systems or Financial Accounting. Desired primary teaching specialties for the second position include Auditing, Forensic Accounting, or Financial Accounting. The Department has recently implemented a Minor in Information Management, as well as Master's programs in both Accounting and e-Business. Additional information may be found at http://business.uwyo.edu/Accounting/default.htm. Please submit a cover letter stating interest in the position, a *curriculum vitae*, summaries of teaching performance, and have three letters of reference sent to Gary M. Fleischman, Chair of Faculty Recruiting Committee, University of Wyoming, Department of Accounting, College of Business, PO Box 3275, Laramie, WY 82071-3275; Email: gflash@uwyo.edu. The University of Wyoming is an EEOC/AA Employer.

425 KENT STATE UNIVERSITY invites applications for an Assistant Professor of Accounting in the teaching and research area of Accounting Systems; anticipated availability of position is Fall 2002. A Ph.D. with a major in Accounting or an appropriate related field is required; professional certification such as CPA, CMA, or CFM is highly desirable, as is professional certification in the systems area. It is preferred that applicants have their doctorate completed now or expect to have all degree requirements completed by Summer 2002. Candidates who are ABD will be considered. Systems research will be expected in high-quality, academic-refereed journals; some publication in other journals acceptable. Participation in teaching doctoral students and serving on and/or supervising their dissertations possible. Contact Dr. Richard Brown, Professor and Chair; Department of Accounting, 550 BSA; Kent State University; Kent, OH 44242; Phone: (330) 672-2545; Fax: (330) 672-2548; Email: dbrown@bsa3.kent.edu.

426 UNIVERSITY OF NEW ORLEANS invites applications for anticipated Assistant Professor positions for Fall 2002. Preference will be given to those with a primary teaching/research interest in Accounting Information Systems or Cost/Managerial, with a secondary interest in Auditing or Financial Accounting. Candidates must have the Ph.D. with an Accounting major (or ABD with completion of the Ph.D. by the end of 2002). Salary is negotiable and competitive. The Department offers the undergraduate Accounting degree (over 500 majors) and two Master's degrees (including a Tax Option). The Department has separate AACSB Accounting accreditation at both the graduate and undergraduate levels. Candidates should send a resume to Professor Philip J. Harmelink, Chairman, Department of Accounting, College of Business Administration, University of New Orleans, New Orleans, LA 70148; Email: pharmeli@uno.edu. The University of New Orleans is a member of the Louisiana State University System and an Affirmative Action/Equal Opportunity Employer.

427 LEHIGH UNIVERSITY seeks a well-trained scholar who also has a strong commitment to teaching to join its Accounting faculty on a tenure track in Fall 2002, and begin publishing in major refereed journals. The opening is for an Assistant, Associate, or Full Professor of Accounting. A new Dean with national aspirations, an emerging M.S. in Accounting program, and a dynamic accounting department make this a great time to consider Lehigh. Our greatest need is in Accounting Information Systems. Applications will be accepted until the position is filled. Qualified women and minority candidates should apply. If interested, send your *curriculum vitae*, three references, and dissertation proposal or other scholarly work to Dr. Kenneth P. Sinclair, Chair, Department of Accounting, Lehigh University, 621 Taylor Street, Bethlehem, PA 18015-3117; Email: kps1@lehigh.edu. Lehigh University is an Equal Employment/Affirmative Action Employer.

428 UNIVERSITY OF WISCONSIN–LA CROSSE announces a tenure-track position at the rank of Assistant/Associate Professor starting with the 2002–03 academic year. Primary teaching responsibilities will include undergraduate Auditing and Principles of Accounting (Financial and Managerial). UW–L has business accreditation at the undergraduate and graduate levels. Review of applications will begin November 1, 2001 and continue until the position is filled. Minimum qualification: doctorate in Accounting. Professional certification is highly desirable. Send application packet to Ken Winter, 405 Wimberly Hall, University of Wisconsin–La Crosse, 1725 State Street, La Crosse, WI 54601; Phone: (608) 785-6830; Fax: (608) 785-8549; Email: winter.kenn@uwlax.edu. Packet must include: (1) letter of application (include demonstrated interest in teaching auditing); (2) undergraduate and graduate transcripts; (3) teaching evaluations; (4) name, phone number, and email address of three references; (5) *curriculum vitae*; and (6) evidence of research (often from dissertation). AA/EOE.

429 NAVAL POSTGRADUATE SCHOOL, Graduate School of Business and Public Policy, seeks applicants for a tenure-track or visiting Accounting faculty position. Candidates should possess a Ph.D./D.B.A. (or be near completion), have excellent teaching ability, and a commitment to scholarly research. Candidates at the Associate or Full Professor rank should have an established research record. Preference will be given to candidates with interests in Managerial or Financial Accounting. Responsibilities include graduate-level teaching of core and advanced courses, master's project advising, and research. The Graduate School of Business and Public Policy offers AACSB- and NASPAA-accredited Master of Science degree programs in Management, with plans to offer an M.B.A. starting 2002. Students include U.S. and international military officers, federal managers, and civilians. Send *curriculum vitae* and names of three references to Professor Kenneth J. Euske, Dean, Graduate School of Business and Public Policy, Naval Postgraduate School, 555 Dyer Road, Monterey, CA 93943. The Naval Postgraduate School is an Equal Opportunity Employer.

430 WESTFIELD STATE COLLEGE invites applications for a full-time, tenure-track faculty position beginning September 2002. It is anticipated that this position will be filled at the rank of Assistant Professor. Salary is dependent upon qualifications and experience. The ideal candidate should have a Ph.D. in Accounting (ABD considered), professional certifications, and some business and teaching experience. Candidates should be prepared to teach from such areas as Principles, Intermediate, and Tax. Review of applications will begin immediately, and continue until filled. Send a cover letter; *curriculum vitae*; names, addresses, telephone numbers, and email addresses of three references; and a transcript of highest degree to Dr. Bradford Knipes, Chair, Economics and Management, Westfield State College, Westfield, MA 01085-1630; or apply by email: bknipes@wisdom.wsc.ma.edu. We are looking for a faculty member who can provide a blend of teaching, scholarship, and service. Women, persons of color, and persons with disabilities are encouraged to apply. AA/EOE.

431 UNIVERSITY OF WISCONSIN–WHITEWATER, Accounting Department, anticipates openings for two full-time, tenure-track Assistant/Associate Professor positions for Fall 2002. The minimum qualifications are an earned terminal degree (Ph.D. or D.B.A.) in Accounting (ABD considered), and the ability to teach introductory Financial or Managerial Accounting. Responsibilities include teaching undergraduate and/or graduate Accounting courses, along with advising, service, and research. The ability to teach Intermediate Financial Accounting is desirable, as are professional certification (e.g., CPA, CMA) and practical experience. An interest in teaching Internet courses is a plus. Specific responsibilities for the first position include teaching Accounting Information Systems; responsibilities for the second position could include teaching Taxation. Salary is competitive and negotiable. All interested persons should apply in writing to Dr. Robert Gruber, Department of Accounting C5019, University of Wisconsin–Whitewater, 800 West Main Street, Whitewater, WI 53190-1790.

432 CLARION UNIVERSITY, College of Business Administration, Department of Accountancy, invites applications for two tenure-track positions beginning August 2002. A doctorate in Accounting is required, but consideration may be given to advanced ABD candidates. Primary teaching responsibilities will be in Managerial/Cost, Advanced, and/or International Accounting. Research and publication, completion of a successful interview, and excellent teaching and service are required. Clarion University is fully accredited by the AACSB. Review of applications will begin November 1, 2001. Send *curriculum vitae* to Dr. Thomas W. Oliver, Chair, Department of Accountancy, College of Business Administration, Clarion University, Clarion, PA 16214; Email: toliver@clarion.edu. Clarion University is a diverse academic community and encourages people of color, women, veterans, and persons with disabilities to apply.

433 UNIVERSITY OF BALTIMORE, Robert G. Merrick School of Business, invites applications for an Accounting faculty position at the Full or Associate rank with the possibility of becoming Division Director beginning in Fall 2002. All areas of specialization will be considered. Teaching assignments will be at the graduate and undergraduate levels. An earned doctorate in Accounting or Business and prior teaching experience are required. Professional certification is desirable. Quality classroom instruction and publications are required for tenure. Salary and benefits are competitive. Candidates should submit a letter of interest with a *curriculum vitae* to Phillip J. Korb, Merrick School of Business, University of Baltimore, 1420 North Charles Street, Baltimore, MD 21201; Email: pkorb@ubmail.ubalt.edu. UB, a University System of Maryland institution, is the state's only upper-division university offering programs in Business, Law, and Liberal Arts. Undergraduate and graduate programs are accredited by AACSB; the graduate program includes a Web-M.B.A. curriculum. AA/EOE/ADA.

434 PENNSYLVANIA STATE UNIVERSITY, Capital College, the AACSB-accredited School of Business Administration, invites applications for a tenure-track Accounting position at Assistant or Associate Professor level beginning Fall 2002. Doctorate in Accounting from AACSB-accredited school is required (ABDs will be considered). Teaching load 9/9 following 9/6 first two years. Responsibilities include teaching of graduate/undergraduate courses in Financial Accounting; an additional area of Accounting expertise is preferred. Primary responsibilities are in Harrisburg, with possible teaching at Schuylkill Haven campus. A strong commitment to both effective teaching and publication is required. Professional certification (CMA, CPA, or equivalent) and prior teaching, research, professional experience are preferred. Compensation is AACSB-competitive. Reviews begin October 31, 2001, continuing until position is filled. Send *curriculum vitae* and references to Accounting Search Committee, c/o Mrs. Dorothy Guy, Penn State Harrisburg, 777 W. Harrisburg Pike, Box AR, Middletown, PA 17057-4898. Penn State is committed to affirmative action, equal opportunity, and diversity of its work force.

435 INDIANA UNIVERSITY KOKOMO invites applications for a tenure-track faculty Assistant Professor position in Business Law and Taxation, August 2002. Requirements include either J.D. with C.P.A. and L.L.M. in Taxation or J.D. with M.S. in Accounting. Active research agenda and evidence of effective teaching are required. Must have capability to teach Business Law and Taxation primarily in the undergraduate program. Professional experience is desirable. Salary is competitive; excellent benefits. One of eight Indiana University campuses, Indiana University Kokomo is a comprehensive nonresidential campus located 50 miles north of Indianapolis in Kokomo, Indiana. Additional information can be accessed at http://www.iuk.edu/. Send a letter of application, resume, and list of three references to Search and Screen Committee, School of Business, Indiana University Kokomo, PO Box 9003, Kokomo, IN 46904-9003; Phone: (765) 455-9275; Email: tbutler@iuk.edu. Review of applicants begins November 1, 2001 and continues until position is filled. IUK is an Equal Opportunity, Affirmative Action, ADA-compliant employer.

436 UNIVERSITY OF ILLINOIS, Department of Accountancy, is recruiting faculty for tenure-track appointments at all ranks. Candidates at advanced ranks must possess a doctoral degree, and candidates for Assistant Professor positions must possess or be near completion of a doctoral degree. Applicants for senior positions should have a strong research record and the ability to participate in doctoral education and curriculum development at the undergraduate and graduate levels. In addition, nontenure-track lecturer and visiting positions may become available. Applicants should submit a resume that lists research publications and courses taught. Compensation is competitive and will be commensurate with the appointment level and applicant's record. To ensure full consideration, applications should be received by December 31, 2001. Proposed starting date is August 21, 2002. Correspondence and resumes should be sent to Professor Gene Willis, Head, Department of Accountancy, 360 Wohlers Hall, College of Commerce and Business Administration, University of Illinois, 1206 S. Sixth Street, Champaign, IL 61820. The University of Illinois is an Affirmative Action, Equal Opportunity Employer.

437 THE UNIVERSITY OF TEXAS AT ARLINGTON, Department of Accounting, invites applications for one tenure-track position at the Assistant/Associate/Full Professor level in the areas of Auditing, Financial Accounting, or Accounting Information Systems. The position requires a Ph.D., high-quality teaching, publishing in nationally recognized academic journals, and academic/professional service; position is available beginning in Fall 2002. Screening of applicants begins immediately and continues until the position is filled. Send an application letter and *curriculum vitae* to Jeff Tsay, Recruiting Committee Chair, U. T. Arlington, Box 19468, Arlington, TX 76019; Phone: (817) 272-6009; Email: jefftsay@uta.edu. U. T. Arlington is located in the dynamic Dallas/Ft. Worth metroplex area. Both the College and Department of Accounting are AACSB-accredited. UTA is an Equal Opportunity and Affirmative Action Employer.

438 UNIVERSITY OF RHODE ISLAND invites applications for an Assistant Professor of Accounting position. Ph.D. or D.B.A. in Accounting is required at time of appointment or must have expected completion date of August 2002. CPA and other certifications are desired. Preference will be given to candidates teaching Auditing, Information Systems, and Managerial Accounting. Candidates must be committed to effective teaching, scholarly and professional research, and university and community service. We anticipate a tenure-track position beginning in the Fall 2002. Review of applications will begin on 9/15/01 and continue until the position is filled. Please send *curriculum vitae*, official transcript, and three references to Mark Higgins, Search Committee Chair, (log#AR041078), University of Rhode Island, PO Box G, Kingston, RI 02881. The University of Rhode Island is an AA/EEO Employer strongly committed to achieving excellence through increased diversity of its faculty, staff, and students. Women, ethnic minorities, and individuals from under-represented groups are encouraged to apply.

439 UNIVERSITY OF ARKANSAS AT LITTLE ROCK invites applications for two tenure-track positions in Accounting beginning in Fall 2002. Appointments at the Assistant, Associate, or Full Professor level will be considered. Applicants for one position should be qualified in the areas of Auditing and Financial Accounting. Applicants for the second position should be qualified to teach Tax/Cost/Managerial/Financial. Candidates should possess an earned doctorate in Accounting from an accredited institution. Professional certification is highly desirable. Experience in and/or aptitude for web-based or online teaching is a plus. UALR is an AACSB-accredited metropolitan university that offers a competitive salary. The primary mission of the University and the College of Business is teaching, but faculty are expected to be actively involved in scholarly activities and provide service to the profession and community. Applicants should send a *curriculum vitae* and list of references to Dr. Jeffrey L. Hamm, Chair, Department of Accounting, College of Business Administration, University of Arkansas at Little Rock, 2801 S. University Avenue, Little Rock, AR 72204; Phone: (501) 569-3351; Email: jlhamm@ualr.edu. Applicant screening will begin immediately and continue until the position is filled. UALR is an Affirmative Action/Equal Opportunity Employer.

440 CREIGHTON UNIVERSITY invites applications for the position of the John P. Begley Endowed Chair in Accounting, effective Fall 2001. The Chair provides leadership in the field of Accounting for the AACSB-accredited department and serves as a key component in maintaining our pre-eminent program. The Chair works closely with both local and national Accounting educators and professionals. The appointment will be at the Associate or Full Professor rank. Candidates must have an earned doctorate (Accounting). Professional certification is desirable. Candidates should possess a strong record of teaching, research, and publications commensurate with the rank. Teaching and research interests in e-Commerce, Accounting Information Systems, or Auditing is preferred. Send application letter and *curriculum vitae* to Thomas Shimerda, Creighton University, 2500 California Plaza, Omaha, NE 68178; Phone: (402) 280-2615; Fax: (402) 280-5565. Applications will be taken until the position is filled. Creighton University is an AA/EOE Employer, and seeks a diverse faculty committed to contributing to the University's mission.

441 UNIVERSITY OF VIRGINIA, Darden Graduate School of Business Administration, is seeking applications from highly qualified individuals for full-time positions in accounting for Fall 2002. Applicants at all ranks may be considered. A doctorate in Accounting is required with an emphasis in either Financial or Managerial/Cost Accounting. Preference will be given to candidates with credentials that demonstrate a strong student-centered teaching orientation, a commitment and capability to engage in research of significant business issues, and a willingness and capability to teach and work with business executives. The Darden School is a professional school that emphasizes the design and delivery of general management educational programs and research that advances the knowledge and practice of the business community. Send *curriculum vitae* to Professor Richard Brownlee, Darden School, PO Box 6550, Charlottesville, VA 22906. The University of Virginia is an Affirmative Action, Equal Opportunity Employer. The Darden School encourages applications from minorities, women, and individuals with significant international experience.

442 WASHINGTON UNIVERSITY, Olin School of Business, is seeking faculty in Accounting at all levels starting Fall 2002. The qualifications include a demonstrated interest in and capacity to do research publishable in leading Accounting journals and a high level of teaching competence in graduate and undergraduate classes. Candidates should hold a doctorate degree or be very near completion. Salary and faculty support are highly competitive. Each application should include a *curriculum vitae* and a copy of the dissertation proposal (or the completed dissertation); more established scholars should include several samples of their scholarship. Send all materials to Dean Stuart I. Greenbaum, John M. Olin School of Business, Campus Box 1133, Washington University, 1 Brookings Drive, St. Louis, MO 63130-4899. Washington University is an Equal Opportunity/Affirmative Action Employer and welcomes applications from women and minorities.

443 SAN JOSE STATE UNIVERSITY, Department of Accounting and Finance in the College of Business, is recruiting at all academic ranks for Accounting positions to begin August 26, 2002. Applicants should have a teaching interest in one or more of the following areas: Accounting Information Systems, Management/Cost Accounting, and Taxation. Teaching assignments will be at both graduate and undergraduate levels in both traditional and accelerated formats. All applicants must hold a doctorate and be academically qualified under AACSB standards. Professional certifications are desirable. All applicants should demonstrate a commitment to teaching excellence, to quality research, and publications consistent with teaching load and interaction with the profession. The application deadline is November 30, 2001 or until the position is filled. All applicants should submit a letter of interest, *curriculum vitae*, available teaching evaluations, and the names and addresses of at least three references to Dr. Joe Mori, Chair, Department of Accounting & Finance, Business Tower, Suite 850, College of Business, San Jose State University, 95192-0066. Phone: (408) 924-3461; Email: mori__j@cob.sjsu.edu. San Jose State University is an Equal Opportunity/Affirmative Action Employer.

444 SOUTHEAST MISSOURI STATE UNIVERSITY invites applications for the Chairperson in Department of Accounting, Finance, and Business Law position available Fall 2002. Requirements include a Ph.D./D.B.A. in Accounting or Finance or J.D. with M.B.A./M.ACC.; university teaching experience; record of significant and sustained scholarly accomplishments; excellent communication skills; strong interpersonal skills; demonstrated awareness of issues affecting women and minorities and history of working with multicultural populations; and demonstrable commitment to the teacher/scholar model and public and university service. Professional certification (CPA, CMA, CFM, or CFA), practical business/accounting experiences, administrative experience, and experience in a multidiscipline environment are preferred. Send resume, available documentation of administrative experience and teaching effectiveness; information relating to communication and interpersonal skills, copies of journal articles, proceedings, and presentations; evidence of practical business experience; the names and telephone numbers of three references; and all transcripts. Review of applications will continue until the position is filled. Contact Dr. Gary G. Johnson, Search Chair, Southeast Missouri State University, One University Plaza MS 5815, Cape Girardeau, MO 63701; Office: (573) 651-2324; Email: ggjohnson@semovm.semo.edu; Fax: (573) 986-6117. Southeast Missouri State University is an Equal Opportunity/M/F/Affirmative Action Employer. Women and minorities are encouraged to apply.

445 SOUTHEAST MISSOURI STATE UNIVERSITY invites applications for a tenure-track Assistant/Associate Professor in Accounting position available for Fall 2002. Requirements include Ph.D./D.B.A. in Accounting, scholarly accomplishments, strong communication and interpersonal skills, demonstrated awareness of issues affecting women and minorities, and history of working with multicultural populations; also demonstrable commitment to the teacher/scholar model and public and university service. University teaching, professional certification (CPA, CMA, or CIA), and practical Business/Accounting experiences are preferred. Send resume, available documentation of teaching effectiveness; copies of journal articles, proceedings, and presentations; information relating to communication and interpersonal skills; the names and telephone numbers of three references; and all transcripts. Review of applications will begin as received and will continue until the position is filled. Contact Dr. Gary G. Johnson, Southeast Missouri State University, One University Plaza MS 5815, Cape Girardeau, MO 63701; Office: (573) 651-2324; Email: ggjohnson@semovm.semo.edu; Fax: (573) 986-6117. Southeast Missouri State University is an Equal Opportunity/M/F/Affirmative Action Employer. Women and minorities are encouraged to apply.

446 UNIVERSITY OF HAWAII AT HILO, School of Business, invites applications for a tenure-track Assistant Professor of Accounting to teach Introductory and Advanced undergraduate courses in Accounting; advise and counsel students; and perform scholarly research and service as warranted. Ph.D. or D.B.A. with major in Accounting or closely related field is required. (ABDs will be considered for Acting Assistant Professor.) For complete qualifications and application requirements, call Dr. Marcia Sakai at (808) 974-7400 or send email to marcias@hawaii.edu. First review of applicants will begin January 31, 2002 and remain open until position is filled. University of Hawaii at Hilo is an EEO/AA Institution.

447 THE PRINCIPAL FINANCIAL GROUP has an immediate opening for an attorney with experience in Corporate Tax. This attorney will provide tax advice to the member companies of the Principal Financial Group, which engage in business in various states and foreign countries. The position emphasizes Tax Planning and Tax Controversies. Qualifications include at least 4 years experience practicing law, with at least 2 years experience in Corporate Tax. A CPA and/or public company experience is preferred, and knowledge of other languages would be a plus. She or he must be able to work independently and enjoy research and writing. Strong communications skills are a must. The Principal Financial Group offers a competitive total compensation package and a comprehensive benefits package. Please send your resume and cover letter to Principal Financial Group HR-Employment W**3528, Des Moines, IA 50392-0550; Fax: (515) 247-5874 or (800) 388-4740. AA/EEO.

448 UNIVERSITY OF WESTERN ONTARIO, Richard Ivey School of Business, seeks candidates for a tenure-track position, tenured appointment, or limited term position in the area of Management Accounting and Control. The position is available to begin in September 2002. The successful candidate will demonstrate a strong commitment to the practice of Management in both research and teaching. This position is subject to budget approval. In accordance with Canadian immigration requirements, priority will be given to Canadian citizens and permanent residents of Canada. Contact Professor Claude Lanfranconi by telephone: (519) 661-3225 or by email: clanfran@ivey.uwo.ca. The Richard Ivey School of Business, an internationally recognized business school, and the University of Western Ontario are committed to employment equity and welcome applications from all qualified women and men, including visible minorities, aboriginal people, and persons with disabilities.

449 UNIVERSITY OF CINCINNATI, Department of Accounting and Information Systems, invites applications for tenure-track positions in Accounting beginning Fall 2002, contingent upon available funding. Rank is open and all areas of specialization will be considered. Minimum qualifications include Ph.D. in Accounting or ABD with a defended dissertation proposal and completion of degree requirements prior to start date. Candidates must show excellence in research and teaching, and a commitment to professional service. The College of Business is AACSB-accredited at the B.B.A. and M.B.A. levels and offers a Ph.D. In addition to a B.B.A. in Accounting, M.S. degrees in Accounting and Taxation are offered. Submit letter of application, *curriculum vitae*, teaching evaluations, three letters of reference, and a working paper and/or defended dissertation proposal to Professor Jens Stephan, Department of Accounting and Information Systems, University of Cincinnati, Cincinnati, OH 45221-0211. The University of Cincinnati is an Affirmative Action/Equal Opportunity Employer.

450 STATE UNIVERSITY OF NEW YORK AT ALBANY invites applications for the rank of Full/Associate/Assistant Professor of Accounting. The position is subject to budgetary approval. Applications for Full/Associate should demonstrate a proven record of teaching and scholarly research. ABDs will be considered at Assistant Professor level. Preference will be given to candidates specializing in Information Systems or Financial Accounting. Applications can be sent to Professor Saurav Dutta, Department of Accounting, School of Business, SUNY at Albany, Albany, NY 12222; Email: sdutta@csc.albany.edu. Applications will be reviewed beginning August 1, and will be accepted until the position is filled. SUNY is an Equal Opportunity, Affirmative Action Employer and encourages minorities, women, and people with disability to apply.

451 ANGELO STATE UNIVERSITY invites applications for a tenure-track position at the Assistant, Associate, or Full Professor level beginning Fall 2002. Summer school or a research grant is usually available at one-sixth of the nine-month salary. Applicants must hold a doctorate. The teaching load will be 12 hours in the fall and 9 hours in the spring and will include an annual graduate Financial Theory course, an annual graduate course in Financial Statement Analysis and Principles of Accounting. Applications should consist of (1) a cover letter, (2) a current resume, (3) photocopies of undergraduate and graduate transcripts, (4) the ASU Application for Faculty Employment, which is available at http://www.angelo.edu/forms/pdf. Review of applications will begin October 19, 2000, and continue until the position is filled. Contact Dr. Norman Sunderman, Head, Department of Accounting, Economics, and Finance, Angelo State University, San Angelo, TX 76909. Angelo State University is an Equal Opportunity/Affirmative Action Employer.

452 UNIVERSITY OF TENNESSEE AT CHATTANOOGA invites applications for an expected tenure-track Assistant Professor position teaching Taxation beginning Fall 2002. Candidates should have a Ph.D. or D.B.A. in Accounting or have a completion date of August 2002. Salary and benefits are AACSB-competitive. Review of applicants will begin on October 15 and continue until the position is filled. For information, contact Dr. Melanie McCoskey, College of Business Administration, Dept 6206, University of Tennessee at Chattanooga, 615 McCallie Avenue, Chattanooga, TN 37403; Telephone: (423) 755-5253; Email: Melanie-McCoskey@utc.edu. The University of Tennessee at Chattanooga is an Equal Employment Opportunity, Affirmative Action, Title VI and Section 504, ADA Institution. Minority applicants are strongly encouraged to apply.

453 UNIVERSITY OF MISSOURI–COLUMBIA, School of Accountancy, is seeking applications for two tenure-track faculty positions in Accountancy at the Assistant or Associate Professor level in Information Systems and Financial Accounting for Fall 2002. These positions require a completed Ph.D. or D.B.A. with research and teaching interests in Information Systems and Financial Accounting. Faculty members teach six hours per semester and are expected to pursue a program of scholarly research. Applications are encouraged from new Ph.D.s or ABDs nearing completion, and experienced Assistant or Associate Professors. Salary and research support are competitive. Please send application letter and *curriculum vitae* to Dr. Earl Wilson, Director, School of Accountancy, University of Missouri–Columbia, Columbia, MO 65211. Screening will begin November 1, 2001 and continue until the positions are filled. The University of Missouri is an Equal Opportunity, Affirmative Action Employer. Applications from minority and women candidates are strongly encouraged for this position. To request ADA accommodations, please contact the University's ADA Coordinator at (573) 884-7278 (V/TTY).

454 INDIANA UNIVERSITY, Department of Accounting and Information Systems, invites applications for an Assistant Professor effective Fall 2002. Preference will be given to candidates with teaching and research interests in Tax. Candidates with teaching and research interests in Financial Accounting will also be considered. Candidates must possess a doctoral degree and have a strong commitment to excellence in teaching and scholarly research. Compensation levels are competitive and negotiable. Interested candidates should send a current *curriculum vitae*, and evidence of teaching ability to Professor Jamie Pratt, Chair, Department of Accounting and Information Systems, School of Business, Indiana University, Bloomington, IN 47405. Three letters of reference will also be required before an offer of employment can be considered. Applications received before January 1, 2002 will be given full consideration, but the position will remain open until a suitable applicant is found. Indiana University is an Affirmative Action/Equal Opportunity Employer. Minorities, women and persons with disabilities are encouraged to apply.

455 CONCORDIA UNIVERSITY, located in beautiful Montreal, Quebec, Canada, invites applications for tenure-track positions in Accountancy at the rank of Assistant, Associate, or Full Professor, effective June 1, 2002. A doctoral degree (completed or near completion) in Accountancy is required. Salaries are competitive and commensurate with qualifications and experience. These positions are subject to budgetary approval. The Department of Accountancy is the only department in Canada accredited by the AACSB—The International Association for Management Education. The department has 22 full-time faculty members, is one of the largest in the country, and has an established record of excellence in professional Accounting education and academic research. Please send your complete application to Dr. Dominic Peltier-Rivest, Chairman, Department of Accountancy, John Molson School of Business, Concordia University, 1455 de Maisonneuve Blvd. West, Montreal, Quebec, H3G 1M8 CANADA; Telephone: (514) 848-2778; Email: drivest@jmsb.concordia.ca. Concordia University has a policy of employment equity, including affirmative action for women faculty.

456 WICHITA STATE UNIVERSITY invites applications for a tenure-eligible position as Assistant/Associate Professor beginning Fall 2002. Required qualifications are a Ph.D. or D.B.A. in Accounting (ABD considered) for Assistant Professor rank; a Ph.D. or D.B.A. in Accounting and a significant research record for Associate Professor rank, teaching and research interests in Financial Accounting with secondary interests in Accounting Information Systems and/or Auditing, strong commitment to quality undergraduate and graduate teaching and research, commitment to service and interaction with the professional accounting community, and successful experience with, and demonstrable commitment to, working with diverse populations. Full- or part-time experience in a professional Accounting capacity and professional certification are preferred. To ensure full consideration, applications must be received by November 1, 2001. The position remains open until filled. Salary is competitive and the position is contingent upon funding. Applicants should send a *curriculum vitae* and list of references to Bill D. Jarnagin, Director, School of Accountancy, Wichita State University, 1845 Fairmount, Wichita, KS 67260-0087; Phone: (316) 978-6262; Email: bill.jarnagin@wichita.edu. Wichita State University is an Equal Opportunity/Affirmative Action Employer.

457 LONG ISLAND UNIVERSITY, Brooklyn Campus, invites applicants for a tenure-track position at the Assistant/Associate Professor level starting January or September 2002 to teach nine semester hours undergraduate and graduate Managerial Accounting or Auditing/Systems. Ability to teach several different courses and demonstrate excellence in research and service is required. Ph.D. is required (ABDs nearing completion considered); CPA preferred. Send cover letter, *curriculum vitae*, teaching evaluations, and three letters of recommendation to Dr. Ben Lee, Head of Search Committee, Department of Accounting, Long Island University, 1 University Plaza, Brooklyn, NY 11201; Email: Drbenlee@mail.com. Applications will be accepted until position is filled. Long Island University is an AA/EEO Employer.

458 DREXEL UNIVERSITY, Department of Accounting of the LeBow College of Business, invites applications for positions at the Assistant or Associate rank in the areas of Taxation and Managerial/Accounting Information Systems to begin in September 2002. The positions require a doctoral degree in Accounting, but ABDs near completion will be considered. Inquiries and applications should be directed to Dr. Kevin Brown, Department of Accounting, LeBow College of Business, Drexel University, 32nd and Market Streets, Philadelphia, PA 19104-2875.

459 BARUCH COLLEGE OF CUNY, Stan Ross Department of Accountancy, invites applications for anticipated positions in Accounting, Auditing, and Taxation for tenure-track positions at all ranks. Rank and salary are dependent on qualifications. For positions as an Assistant Professor, candidates should have a doctoral degree in Accounting or be close to completion of that degree. For ranks above Assistant Professor, the requirements are a record of publication, demonstrated teaching effectiveness, and a desire to work with doctoral students. Please submit *curriculum vitae* and recent work of a research proposal, by February 15, 2002, to Professor Masako Darrough, Baruch College of CUNY, 17 Lexington Avenue, Box E-0725, New York, NY 10010. Baruch College is an AA/EO/IRCA/ADA Employer.

460 KANSAS STATE UNIVERSITY, Department of Accounting, invites applications for a tenure-track position in Managerial Accounting. The person who fills the position will provide leadership in enhancing K-State controllership track and teach at both the undergraduate and graduate levels. Professional certification and knowledge of Accounting Information Systems are desirable. Commitment to teaching excellence and scholarly research is required. Candidates should have a doctoral degree in Accounting or expect to complete all requirements during the academic year for which he or she is initially hired. Review of applications will begin immediately and continue until the position is filled. Send *curriculum vitae*, a list of three references, and evidence of scholarship (the dissertation proposal is acceptable for candidates with degree in progress) to Professor Dan Deines, Search Chair, Department of Accounting, Calvin Hall 109A, KSU, Manhattan, KS 66506-0502; Email: ddeines@ksu.edu. Kansas State University is an EEO Employer and actively seeks diversity in its pools of applicants.

461 XAVIER UNIVERSITY, a Catholic university in the Jesuit tradition, invites applications for a tenure-track position at the Assistant level in the Department of Accountancy starting in Fall 2002. A Ph.D. in Accounting from an AACSB-accredited program is required. ABDs will be considered if completion of the dissertation is anticipated by the starting date. Teaching assignments include Accounting Principles as well as upper-level Financial Accounting courses. An interest in teaching International Accounting would be a definite plus. The candidate should have a strong commitment to both high-quality teaching and a research agenda that will lead to publication in refereed journals. The Williams College of Business, which is AACSB-accredited, offers an undergraduate major in Accounting and an M.B.A. with an Accounting/Taxation concentration. Salaries are competitive. Please direct a letter of application and *curriculum vitae* to Edward J. VanDerbeck, Chair, Department of Accountancy, Xavier University, Cincinnati, OH 45207, or send email to james@xu.edu, no later than August 10, 2001 if you wish to ensure being considered for an interview at the AAA Annual Meeting in Atlanta. Deadline for applications is November 1, 2001. Xavier University is an Equal Opportunity/Affirmative Action Employer.

462 WESTERN KENTUCKY UNIVERSITY, Ford College of Business (AACSB-international accredited), is seeking applicants for a tenure-track position beginning August 2002. Qualifications include doctorate in Accounting (ABDs near completion also considered). Professional certification is a plus. Applicants should have a record of quality teaching and research. New doctorates should have a demonstrated potential for quality teaching and research. The Department's strongest needs are Governmental, Systems, and Auditing. Teaching excellence is stressed and research is expected and supported. Candidates should document teaching effectiveness and research. Applicants should submit letter of application, *curriculum vitae*, transcripts, supporting documentation, and three letters of reference to Chair, Accounting Search Committee, Grise Hall 500, Western Kentucky University, 1 Big Red Way, Bowling Green, KY 42101. Review of applicants will begin December 1, 2001; position will remain open until filled. More information about the University can be obtained by contacting (270) 745-3895; Fax: (270) 745-5953 or by visiting our web site at http://www.wku.edu/gfcb. Women and minorities are encouraged to apply. Western Kentucky University is an Affirmative Action/Equal Opportunity Employer.

463 QUEEN'S UNIVERSITY, School of Business in Kingston, Ontario, invites applications for two tenure-track positions in Accounting (Financial and Managerial). The effective date is July 1, 2002, but is flexible. Candidates must have a Ph.D. and exhibit high potential for innovative scholarly research and for outstanding teaching contributions in support of the School's academic programs. Queen's University School of Business is AACSB-accredited; more information is available at http://www.queensu.ca. Please submit a cover letter, current *curriculum vitae*, references, and a sample publication, preferably by email to deansoffice@business.queensu.ca, or by mail to Thomas Anger, Associate Dean, Queen's School of Business, Queen's University, Dunning Hall Rm. 140, Kingston, Ontario, K7L 3N6 CANADA. This is an international search, open to candidates of all nationalities. However, in accordance with Canadian Immigration requirements, priority will be given to Canadian citizens and permanent residents. Queen's University is committed to employment equity and welcomes applications from all qualified women and men, including visible minorities, aboriginal people, persons with disabilities, gay men, and lesbians.

464 NORTH DAKOTA STATE UNIVERSITY, College of Business Administration, (AACSB-accredited) invites applications for two tenure-track positions in the Department of Accounting and Information Systems beginning January or August 2002. Rank is open depending upon qualifications. Preferred areas of teaching and research are Internal Auditing, Forensic Accounting, External Auditing, and Tax; individuals with primary interest in other areas of Accounting will be considered. Minimum qualifications include Ph.D. or D.B.A. in Accounting with research and teaching potential (ABD will be considered if applicant is close to completion); good interpersonal skills; demonstrated effective written and verbal communications skills. Preferred qualifications include significant record of publications, teaching experience, and excellent teaching evaluations. The application deadline is November 1, 2001 or until positions are filled. Please send letter of application, *curriculum vitae*, transcripts, teaching evaluations, and three letters of reference to Dr. Chuck Harter, Chair, Accounting and Information Systems, NDSU, Box 5075, Fargo, ND 58105-5075. NDSU is an Equal Opportunity Institution.

465 UPS CAPITAL, an innovative Financial Services subsidiary of UPS, is currently recruiting for the following accounting professionals: Tax Supervisor, requiring an Accounting major with a B.A. degree and 3–5 years experience and knowledge in the preparation of both complex state and federal corporate income-tax returns. Accounting Supervisor, requiring a Finance or Accounting major with a B.A. degree and at least 5 years of accounting-related experience, and knowledge of internal controls for the Financial Services industry. Relocation will not be provided for these opportunities. Please visit our web site at http://www.upsjobs.com for additional information and online job posting.